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SELECTED MONOGRAPHS:

CZERMAK
ON THE
PRACTICAL USES OF THE LARYNGOSCOPE.

DUSCH
ON
THROMBOSIS OF THE CEREBRAL SINUSES.

SCHROEDER VAN DER KOLK
ON
ATROPHY OF THE BRAIN.

RADICKE
ON THE
APPLICATION OF STATISTICS TO MEDICAL ENQUIRIES.

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ON THE

LARYNGOSCOPE,

AND ITS EMPLOYMENT IN

PHYSIOLOGY AND MEDICINE.

BY

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TRANSLATED FROM THE FRENCH EDITION

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WITH CORRECTIONS, ADDITIONS, AND AN
APPENDIX ON RHINOSCOPY,
BY
THE AUTHOR.

THE NEW SYDENHAM SOCIETY,
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MDCCCLXI.

PREFACE

TO THE

ENGLISH EDITION.

THE following pages are, as near as possible, a literal translation, by Dr. G. D. Gibb, of the French edition[1] of my German brochure[2] translated by Dr. L. Mandl, of Paris. I have enlarged the present work by the addition of an Appendix on Rhinoscopy, together with some notes and sketches.

I leave the preface unaltered, as it appeared in the original German, because it brings before the mind of the reader the actual desires which I entertained, and which were subsequently followed out with success.

THE AUTHOR.

PRAGUE, BOHEMIA;
February, 1861.

[1] 'Du Laryngoscope et de son emploi en Physiologie et en Médecine.' Paris, J. B. Baillière; June, 1860.

[2] 'Der Kehlkopfspiegel und seine Verwerthung für Physiologie und Medizin.' Leipzig, Engelmann; January, 1860.

PREFACE

TO THE

GERMAN EDITION.

This little work on the Laryngoscope is, I may say, a second edition of all the articles which I published in various medical journals in 1858 and 1859, in which I made it my study to bring into scientific and practical use the manifold applications of the *principle* of Liston and Garcia's method of inspecting the larynx.

It is destined to keep alive the first public impulse which I gave to the application and improvement of this method in different directions; and to win new friends and fellow-workers towards the enrichment of this valuable means of exact investigation, which has not been, as yet, sufficiently appreciated.

This object will have been attained when the laryngoscope is brought into daily use, like the ophthalmoscope, the speculum vaginæ, the stethoscope, and other instruments; when, everywhere, even a few physicians shall be found who understand the management of this simple instrument in a dexterous and successful manner, and when others—as has in some measure already been done—

roused by my efforts, shall try to apply the principle of the method of Liston and Garcia in all the most *varied* directions of which it is capable.

Claims of priority can never come into consideration where *literary facts* speak for themselves; nevertheless, as they have been raised in a certain quarter, I shall here simply quote the following words of Dr. Locher,[1] namely, "That we do not consider him as the inventor, in the real and beautiful sense of the word, who first conceives and partially carries out a new idea, and then lays it aside without even a presentiment of its importance; but him, on the contrary, who first discovers the practical application of the idea (even when it has originated elsewhere), and helps towards its public recognition."

In conclusion, I offer my most hearty and sincere thanks to all those who kindly aided me with their friendly assistance in this work.

THE AUTHOR.

PESTH, HUNGARY;
November 27th, 1859.

[1] 'The Diagnosis of Diseases of the Lungs by means of Percussion and Auscultation.' Zurich, 1853, p. 52.

ON THE

LARYNGOSCOPE

AND ITS EMPLOYMENT

IN PHYSIOLOGY AND IN MEDICINE.

CHAPTER I.

HISTORY.

The present is not the first occasion that, by the aid of a mirror attached to the end of a stem, and placed at the back part of the mouth, the study of the larynx in the living has been conceived, for the purpose of illustrating its physiology and pathology.

Already in 1840, Liston[1] mentioned the successful employment of this instrument in medical practice; and in 1855, Garcia[2] published a series of laryngoscopic researches as to the formation of the voice.

Nevertheless, it seems that the observations of Liston have been completely forgotten, and that the brilliant results obtained by Garcia, viewed by prejudice, have excited only mistrust and doubt.

[1] 'Practical Surgery,' with 50 engravings on wood, by Robt. Liston, Esq., London, 1840, p. 417: "Ulcerated glottis.—A view of the parts may be sometimes obtained by means of a speculum; such a glass is used by dentists on a long stalk, previously dipped in hot water, introduced with its reflecting surface downwards, and carried well into the fauces."

[2] "Observations on the Human Voice." ('Philosoph. Magazine and Journal of Science,' vol. x, p. 218, and 'Gazette Hebdomadaire,' 16th November, 1855, No. 46.)

Various attempts have since been made, but they have been always very soon abandoned.

Among these fruitless attempts I will cite only those of Dr. Turck, chief physician to the General Hospital at Vienna, who occupied himself, in the wards of the hospital during the summer of 1857, with some researches on the laryngoscope, having reference solely, as he himself stated, to its employment in diagnosis. No one has appreciated at its just value the principle of the method indicated by Liston and Garcia, nor has public attention been sufficiently attracted toward the subject.

As for myself, I did not commence my laryngoscopic studies until the course of the winter of 1857 and 1858, at first with the intention of obtaining a clear idea of the mode of production of certain Arabic sounds, called *true gutturals,* and then to repeat and complete the physiological experiments of Garcia. But I very soon recognised, by inventing a new proceeding (viz., in using artificial light, and the large ophthalmoscope as a reflector), all the practical value of the instrument, which I had begun to handle.

My first experiments were made with oblong quadrangular mirrors, of which the stem was attached to the middle of the narrower edge, by means of a hinge. These mirrors were lent to me by Dr. Turck, after he had ceased the experiments, for which he had made them.

Already in the commencement of April, 1858, I had improved the laryngeal mirrors most essentially by having them made of *different sizes,* with the stem soldered to the mirror itself, and not fastened by a hinge. I soon became convinced, both by what I observed in myself and in others, that the laryngoscopic method would prove of great importance and of varied application.

From that moment I considered it necessary to invite the attention of practitioners to the laryngeal mirror (see 'Bibliography,' No. 1),[1] and to explain the advantages which physiology and practical medicine might receive by its general employment. This was the first appeal to the profession; no person had as yet dreamt of doing so.

Very soon afterwards I made a communication to the I. R. Society of Physicians of Vienna (sitting April 9th, 1858), upon the value and application of the laryngoscope ('Bibliography,'

[1] All the numbers here given appear in the 'Bibliography,' pp. 5, 6.

No. 2), describing certain new physiological observations made by the aid of this mirror. To give more weight and value to my verbal communication, I demonstrated before my auditors the various laryngeal functions upon myself.

At the same sitting, Dr. Turck, who, in consequence of my success, was struck with the importance of the subject which he had prematurely given up, and now intended to resume, made some remarks relative to the manipulation of the laryngoscope.[1]

[1] 'Zeitschrift d. Ges. d. Aerzte,' zu Wien, No. 17, 26th April, 1858. Sitzungsbericht, p. 271.

"Professor Czermak delivered an open lecture on the laryngoscope with demonstrations. The lecture was already (April 17th) published."

"Dr. Turck, who had already been engaged with experiments, during the past summer, relative to the employment of the laryngeal mirror upon certain individuals in the wards of the General Hospital, for diagnosis, claims, in relation to an article published in the thirteenth number of the journal, 'Wiener medizinische Wochenschrift,' of the 27th March, of the current year, the priority of the application of the laryngeal mirror to diagnosis. He cites as the first attempt the following method, which alone renders possible the exploration of the larynx and neighbouring parts, in a large number of individuals.

"Many persons cannot support the pressure of the mirror against the veil of the palate; Dr. Turck obviates this, as much as possible, in reserving a space for the introduction of the mirror after having caused the tongue to be protruded, so as to produce a hollow on its surface, of which the concavity is directed upwards; the respiration is not interrupted.

"At the same time that the tongue is projected forwards, the larynx becomes elevated in some measure, previous to the exploration. It is oftentimes of advantage to place the mirror obliquely.

"The speculum which Dr. Turck employs is composed of a mirror, small, oblong, well rounded off; of a straight stem, attached at an obtuse angle, but this varies in consequence of the flexibility of the metal; and of a straight handle, which represents the continuation of the stem.

"Dr. Turck wishes to observe, in conclusion, that he is *very far from having any exaggerated hopes about the employment of the laryngeal mirror in practical medicine;* that there will be given in this journal a detailed description of his method, which is here already pointed out."

I cite this literally to show how unfounded are Dr. Turck's claims of priority, for at that time he could not bring forward any other than those few shallow observations; whereas I had already obtained, by my improved method, a series of physiological results, and had publicly recommended its *general* application in physics, and not solely for the diagnosis of diseases of the larynx.

Although Dr. Turck was greatly encouraged by Professor Bruecke and others, not to lay aside the laryngoscope, he nevertheless entirely abandoned his attempts, after Garcia's simple method, made during the summer of 1857. He did this, discouraged by difficulties which he could not overcome, and with-

Afterwards, on April 29th, 1858, I presented to the Imperial Academy of Sciences of Vienna ('Bibliography,' No. 3), my

out even recognising the importance of the method which he had not made any *real* application of.

As to his later and more successful experiments, he would not have performed them, were it not for the impulse which I had given to the whole subject.

That such is the *fact*, any person may convince himself by a close examination of the contents and date of our published results, in which it will be seen, that invariably his publications follow my own (see 'Bibliography'). Also by a glance at the two following letters from Professor Bruecke, of Vienna.

In these letters addressed to me, Professor Bruecke thus expresses himself:

"My honorable Friend,

"I confirm what you have stated, that Dr. Turck having communicated to me his first researches on the laryngoscope, I subsequently inquired if he continued them, *and I requested him not to abandon them.*

"After such a length of time, I cannot remember if that took place once or twice. I cannot even state exactly the reply of Dr. Turck, but I was under the impression *that he had abandoned the question; he positively communicated to me no further results.*

"Whilst very much regretting to see a discussion continued between two learned men whom I esteem in a very high degree, *I authorise you, being the party assailed, to make whatever use of this letter you may consider convenient, if you believe it necessary to establish your rights.*

"Yours,

(Signed) "E. BRUECKE.

"Vienna, Nov. 13th, 1859."

"Dear Friend,

"A few days ago Dr. Turck visited me respecting the letter which you published, and requested a declaration from me, to the effect that I had not encouraged him to employ the laryngoscope *for practical purposes.*

"I gave him the following written declaration, made according to the best of my recollection: 'In reference to my letter published by Professor Czermak ('Du Laryngoscope et de son emploi en physiologie et en médecine,' Paris, J. B. Baillière, 1860, p. 106) you wished me to declare, that I had not encouraged you to employ the laryngoscope for practical purposes, the following is my reply. My request to you, not to lay aside the laryngoscope, as near as I can recollect, was meant *quite generally, without any special hint;—practical use, diagnosis, etc. etc., were altogether out of the question.*

"Dr. Turck thought he could remember having answered me to the effect that, as he knew Garcia's work, experiments made on healthy persons had not any further interest for him, but that he was engaged in its application for the purpose of diagnosis.

"*This I was compelled to deny most emphatically,* because I should most assuredly have remembered such a distinction having been made between

physiological observations made by the aid of the laryngoscope, the details of which were illustrated by drawings.

The impulse which I had thus given to the subject has not remained fruitless, as is proved by the labours of others in 1858 and 1859, recorded in various scientific journals.

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3. Czermak.—Physiological Researches with the laryngeal mirror of Garcia, with 3 plates (*Comptes rendus des séances de l'Académie impér. de Vienne*, vol. xxix, No. 557, April 29th).
4. Semeleder.—On the employment of the laryngeal mirror in the diagnosis and treatment of diseases of the tongue, communicated at the sitting of physicians of Vienna, May 28th (*Zeitschrift der Gesellschaft der Aerzte zu Wien*, No. 28).
5. Turck.—On the laryngeal mirror and its mode of employment, with engravings on wood (*Zeitschrift der Gesellschaft der Aerzte zu Wien*, No. 26, June 28th; *Gazette hebdomadaire de Paris*, February 4th, 1859).
6. Dr. Neudoerfer proposes, in describing a new canula for laryngotomy, to explore the larynx and trachea by the aid of a speculum introduced through the wound (*Zeitschr. für pract. Heilkunde, Wien*, No. 46, November 12th).
7. Stoerk.—On the value of the laryngo-pharyngeal mirror. Note of two pathological cases (*Zeitschrift der Gesellschaft der Aerzte zu Wien*, No. 51, December 20th).

1859.

8. Czermak.—Communication relative to the surprising discovery of a small polypus situated upon the right vocal cord, in an individual affected with hoarseness, said to be "nervous" (*Gazette hebdomadaire de Vienne*, No. 2, January 8th).

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physiology and pathology, especially when the dispute between you and him occurred.

"At the same time I stated to him, in a friendly, but perfectly distinct manner, my candid opinion about the entire proceedings.

"Yours,

(Signed) "E. Bruecke.

"Vienna, Oct. 18th, 1860."

9. CZERMAK.—Researches on laryngoscopy, with engravings on wood, published in Hungarian (*Gazette hebdomadaire de Pesth*, No. 2, February 20th). The first series with seven pathological cases.
10. TURCK.—Upon an art in the examination of the larynx (*Zeitschrift der Gesellschaft der Aerzte zu Wien*, No. 8, February 21st).
11. CZERMAK.—The original text in German, of the seven pathological cases, previously translated into Hungarian (No. 9) (*Wiener Mediz. Wochenschrift*, No. 10, March 5th, and following numbers).
12. Dr. TURCK communicated to the Society of Physicians of Vienna (sitting of March 11th) a series of pathological observations, likewise seven in number, and made with the aid of the laryngoscope (*Zeitschrift der Gesellschaft der Aerzte zu Wien*, No. 11, March 14th).
13. TURCK.—On the laryngo-pharyngeal mirror, and its employment in diseases of the larynx and neighbouring parts (*Allgemeine Wiener Medizinische Zeitung*, No. 15, April 12th, and following numbers).
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15. GERHARDT, of Tubingen.—On the employment of the laryngeal mirror (*Archiv für physiologische Heilkunde*, vol. iii, p. 420).
16. CZERMAK.—On the inspection of the pharyngo-nasal vault, and of the nasal cavity, by means of small mirrors (*Wiener méd. Wochenschrift*, No. 32, August 6th).
17. SEMELEDER.—On the illumination of the larynx (*Allgemeine Wiener mediz. Zeitung*, No. 14, October 4th, p. 305).
18. STOERK.—On the laryngoscope, I (*Zeitschrift der Gesellschaft der Aerzte zu Wien*, No. 46, November 14th).
19. TURCK.—Upon an apparatus intended for the artificial illumination and examination of the posterior wall of the larynx (*Allgemeine Wiener med. Zeitung*, No. 48, November 29th).

Whilst the German edition of my memoir was being printed, which appeared in January, 1860, the following articles were published:

20. TURCK.—Communication on the laryngoscope (*Comptes rendus des séances de l'Académie imp. de Vienne*, sitting of December 9th).
21. TURCK.—On the manner of obtaining the laryngoscopical image enlarged (*Zeitschr. der Ges. der Aerzte zu Wien*, No. 32).
22. STOERK.—On the laryngoscope, II (*Zeitschr. der Ges. der Aerzte zu Wien*, No. 52).

CHAPTER II.

DESCRIPTION OF THE MODE OF EXAMINATION OF LISTON AND GARCIA.

The principle of the method of Liston and Garcia, which renders the larynx and neighbouring parts accessible to view in the living, is remarkably simple.[1]

A small, flat mirror, with a long stem, previously warmed to prevent its being tarnished by the breath, is introduced into the mouth, widely open, as far as its back part. It is then held up in such a manner as to permit the rays to penetrate it, on the one hand, and consequently to illuminate those parts which it is desirable to examine; and on the other hand, the image of those parts is reflected into the eye of the observer. It is understood that the objects reflected are reversed; and consequently the right vocal cord appears on the left side, in the same manner as the right hand of any person placed before a looking-glass is found to be on the left side of the figure.

When a person wishes to examine himself, it is requisite to employ a second flat mirror, according to Garcia, which reflects the image of the subjacent parts represented by the laryngeal mirror. This image, twice repeated, is symmetrical, according to the nature of the object; thus the right vocal cord appears to the right, and so on.

In any particular instance (see note on next page) when it is desired to interrupt the course of the luminous rays several times, we may employ two mirrors instead of one only, as I have already recommended ('Bib.,' Nos. 1 and 16).

[1] Garcia, loc. cit. "The method which I have adopted is very simple; it consists in placing a little mirror, fixed on a long handle suitably bent, in the throat of the person experimented on, against the soft palate and uvula. The party ought to turn himself towards the sun, so that the luminous rays falling on the little mirror may be reflected on the larynx. If the observer experiments on himself, he ought, by means of a second mirror, to receive the rays of the sun and direct them on the mirror which is placed against the uvula."

It is understood that the mirrors cannot represent any other image than that of the parts that lie uncovered in the axis of vision, and in the direction of the luminous rays.

From what has been just stated, it follows, that—

1. It is necessary that the various organs of the mouth and pharynx should be placed in a suitable position; this can be accomplished more or less perfectly, either by changing the position of the neck and head, or by causing certain movements to be performed by the person examined (such as a sudden deep inspiration, the intonation of vowels, especially of the sound *eh !*, singing, laughing, &c.,) or in using various mechanical means (as the employment of a tongue depresser, or manual aid of an assistant, &c.).

2. The mirror must be maintained at a certain angle in a suitable place at the back part of the mouth, by the aid of its long stem. It is understood that the surface of the mirror should be turned at the same angle towards the eye of the observer, as to the parts being examined.

3. The visual axis of the observer should be placed in the same direction as the bright rays which illuminate the laryngeal mirror; this may be easily managed by the aid of a pierced illuminating mirror (see page 15, Fig. 5).

In fact, we may succeed in seeing in this way, in the living, the deep portion of the pharynx, the parts which form the larynx, and, if the passage through the glottis is widely open, the cartilaginous rings of the trachea projecting from their mucous membrane; we may perceive likewise, as I have demonstrated for the first time upon myself (see Chap. IV, end of Section I), the bifurcation of the trachea.

It is unnecessary to remark that the whole of this region cannot always be examined with the same facility, nor to the same extent; because there are certain places more difficult to see than others,[1]

[1] Garcia has seen only the two posterior thirds of the vocal apparatus. In my first experiments I succeeded in seeing more of them; but I then believed that it was necessary to use two mirrors, inclined one against the other, or one convex mirror with a short radius, in order to perceive the anterior point of insertion of the vocal cords. Dr. Turck (No. 5) is the first who remarked, with justice, that we can see the whole of the posterior surface of the epiglottis, and the anterior angle of insertion of the vocal cords, by the aid of the simple laryngeal mirror. On the other hand ('Bib.,' No. 19), profiting by the system of double mirrors, proposed by myself, he has described a particular disposition of two mirrors, by the aid of which the posterior walls of the larynx are perceived with great facility, and to a much larger extent.

or which have not as yet been examined, such as, for example, the posterior wall of the trachea. The parts which may be observed in the *greater number of instances*, if the observer is *skilful and persevering*, and which are completely invisible, or are rarely or never seen but with difficulty, without the aid of the laryngoscope, are—the base of the tongue, the walls of the pharynx, the epiglottis, the aryteno-epiglottic ligaments, the arytenoid cartilages, the true and false vocal cords, the ventricles of Morgagni, the anterior wall of the larynx, and lastly, a portion more or less considerable of the anterior wall of the trachea.

These facts are sufficient to prove that the method proposed by Liston and Garcia is one of great importance for correct observation in physiology and medicine; but the value of this proceeding acquires even higher importance in its application. Again, as I have already observed in regard to it, in my first essay ('Bib.,' No. 1), "The eye directs the hand of the operator in those regions habitually concealed from view (see Chapter III, Section VI). On the other hand, this principle again meets with a wide application in correctly examining other parts inaccessible to the eye." (See the Examination after Laryngotomy, Chapter V, cases 1 and 3; and the Inspection of the Posterior Orifice of the Nasal Cavity, Chapter III, Section V.)

Nevertheless, in spite of the simplicity of the principle, many obstacles and difficulties present themselves against the advantageous employment of the laryngoscope, and its correct appreciation by physiologists and physicians; even actually notwithstanding the numerous proofs of its application, many persons become discouraged after certain fruitless attempts, as did my predecessors up to the time of Garcia.

These difficulties and obstacles result in part from the excitability, sometimes very considerable, at the back part of the mouth, on its coming into contact with a foreign body; from the difficulty which many persons experience in opening their mouths wide enough, and of mastering the movements of the tongue; also from the conformation and unfavorable disposition of organs; and in fine, chiefly *from the inexperience and awkwardness of the investigator.*

The introduction of the mirror with facility and confidence, and consequently without any unusual excitement of the back part of the mouth; its prompt application to the most favorable and least sensitive spot; the habit of detecting and recognising the reflected

image, particularly of those regions but little known, and when the parts are moveable; the directions to be given to the individual undergoing examination, to perform certain movements, and to assume the attitude necessary to the most favorable arrangement of the various parts of the mouth and pharynx; in fine, the regulation of the light and of the visual direction—all these circumstances require a degree of practice and dexterity, which can be attained only by *great perseverance*, conjoined with some preliminary knowledge and some amount of natural skill.

Those circumstances will always be very grave obstacles for the beginner, and will cause those physicians to hesitate in the employment of the laryngoscope, who do not intend to make a *speciality* of laryngoscopy.

Many other methods of examination, however, offer the same difficulties at the commencement, but they do not in the least detract from their value; as an instance I shall here cite the ophthalmoscope, which certain persons only have learned to use with success.

CHAPTER III.

OF THE LARYNGOSCOPIC INSTRUMENTS AND THEIR EMPLOYMENT.

SECTION I.—*The Laryngeal Mirror.*

THE laryngeal mirror employed by Liston and Garcia possesses a flat surface, and is attached to the end of a long stiff handle.

I have given to the mirror a square shape, with the angles rounded off (Fig. 1), and have had the stem soldered to one of its four corners ('Bib.,' No. 2). Dr. Turck afterwards employed round or ovoid mirrors ('Bib.,' No. 5).

FIG. 1.

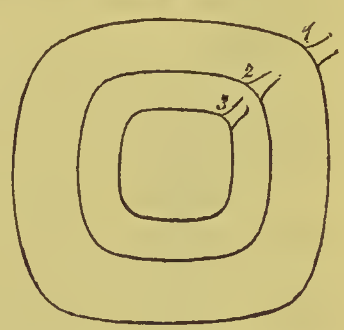

Fig. 1. The outline of three different models of the laryngeal mirror; the insertion of the stem is indicated by 1, 2, 3.

The diameter of the mirror is of more importance than its *shape*, in consequence of the varying irritability and size of the buccal and pharyngeal cavities in different individuals. Besides, we can readily understand why it is preferable to employ large rather than small mirrors; because the brilliancy of the light, and extent of the visual plane, increase with the dimensions of the reflecting surface.

I recommend mirrors of dimensions varying from four lines to an inch and a quarter ('Bib.,' No. 2). Since the beginning of April 1858, in observations made upon myself, I have used a glass of the relatively large size represented in Fig. 1, 1.

The *medium mirrors*, of a diameter of from six tenths to eight tenths of an inch, are the most suitable for common use; nevertheless there are circumstances in which we may employ, with success, mirrors sometimes of greater, and sometimes of smaller dimensions.

The *thickness* of the mirror is about the fifteenth part of an inch. I have found it convenient, in later experiments, to employ mirrors of which the thickness diminishes from the point of attachment to the

stem, where they are the strongest, towards all other points; I do not, however, attach any very great importance to this.

The *whole mass* of the mirror possesses a relative value, because it determines partly the time during which the reflecting surface preserves the temperature necessary for preventing its being tarnished by the condensation of aqueous vapours.

The speedy cooling and tarnishing of the mirror are always troublesome at the commencement of laryngoscopic studies, so long as we can only determine by repeated trials a suitable place for its position.

For the purpose of preventing the cooling of the mirror as much as possible, it is generally necessary to impart to it as high a temperature as can be borne without inconvenience by the person under examination.

A less elevated temperature suffices to prevent the tarnishing of the mirror for a sufficient length of time, when its bulk is considerable.

As soon as the mirror is getting cool, we observe a deposit of moisture upon it at each expiration, which at first is more or less dispelled at each inspiration.

We heat the mirror, either by plunging it in hot water, thus following Liston's plan, or by holding it above a flame which does not smoke, exposing not the back, but the surface only, to the flame, as I have elsewhere pointed out ('Bib.,' No. 1).

The mirror ought to be of glass or speculum metal, and better still of steel.

Glass mirrors are usually mounted in metal, but it is preferable to choose for this purpose bodies that are bad conductors of heat. This mounting becomes useless for metallic mirrors. At all times it will prove of advantage to encase the reverse of the mirror with a substance which is a bad conductor. By this means the cooling of the mirror could be retarded, and at the same time it could be brought to a higher degree of temperature, without inconveniencing the person who is being examined.

The stem which is attached to the mirror should possess a certain degree of stiffness, and a length of from three to four inches, from the point of its insertion up to the handle.

The obtuse angle, at which the mirror is inclined upon the stem, is situated in a plane running perpendicularly to the surface of the mirror through that of its two diagonals, which contains the point

at which the stem is attached. Thus the mirror can be used indifferently either with the right or left hand.

Notwithstanding the rigidity of the stem, it can be bent at pleasure, and thus enable us to alter the angle formed between it and the mirror, or even to give a slight curvature (Fig. 2), if it is required for the introduction and support of the mirror.

But we may be permitted to make the following remark, in conclusion. The success of the examination will depend very much less upon all the foregoing details, concerning the construction of the mirror, than upon the dexterity and skill of the observer.

It is easy, with these data, to construct the small laryngeal mirrors everywhere, at a trifling expense.[1]

FIG. 2.

Fig. 2. Laryngeal mirror of steel, of medium size, with its flexible stem, and commencement of the handle.

SECTION II.—*Of the Light.*

Garcia recommends the illumination of the organs by the aid of the light of the sun, directed upon the mirror introduced into the back part of the mouth of the person under examination. This individual should be placed fronting the sun, so that the observer himself could turn his back towards it, and allow the rays of light to pass by the side of his head; the visual axis of the observer ought to be as much as possible parallel to the course of the incident rays.

But it might so happen that the observer throws a shadow upon the laryngeal mirror, or that he is looking in a direction that is not at all, or scarcely illuminated.

[1] These mirrors are made in Paris, according to my directions, by M. Mathieu, surgical instrument maker, and by Weiss and Son, 62, Strand, London.

If, for example, as is shown in Fig. 3, *k* represents the laryngeal mirror, *d* a fasciculus of the solar rays, *c* the visual axis of the observer; the point *a*, which remains invisible to the observer, is found illuminated, whilst the point *b*, accessible to view, remains obscure.

Fig. 3.

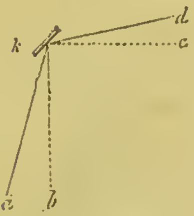

Fig. 3. *k*, Laryngeal mirror; *bk*, *ck*, visual rays; *ak*, *dk*, luminous fasciculi.

The position of the observer, and of the individual examined, ought equally to vary according to the height of the sun; this sometimes produces situations unfavorable to the success of the examination.

Finally, the observer may find himself baffled in a very unfortunate way, by the situation of the apartment, by the more or less clearness of the sky, and by the changes in the direction of the rays of the sun at the different hours of the day, &c.

At the commencement of my laryngoscopic studies, I very naturally conceived the idea of substituting for the light of the sun, which is so frequently at fault, an artificial light, which would permit of uninterrupted observations. I soon succeeded in showing that we could do without the rays of the sun in our laryngoscopic researches, up to a certain point, although we might never wholly replace them.

If we approximate an intense flame to the open mouth of a patient, the observer can examine the pharynx and employ the laryngoscope, in placing his eye very close to the flame; and this latter may be masked by a screen or a reflector, as in the simple form of ophthalmoscope of Helmholtz. In order to have both hands free, I have placed a candle and its reflector before the eyes, by means of a handle fixed between the teeth ('Bib.,' No. 2).

But I have succeeded in obtaining a result more complete and easy still ('Bib.,' No. 1), in using the large ophthalmoscope of Ruete, joined to a powerful moderator-lamp.[1]

On introducing the employment of the ophthalmoscope in laryngoscopy, I have accomplished the necessary concentration of the rays of artificial light, in a manner by which these researches have become independent of the light of the sun. But I have found it very convenient, on the other hand, always to use for the purposes of

[1] Since then I have used a whiter and more intense light, such as, for example, that of ordinary gas, camphine, &c. Besides, natural philosophy furnishes us with sufficient means for increasing the intensity of artificial light.

light, reflecting mirrors pierced by a hole in their centre, even when we have the light of the sun at our disposal; in that case, the concave mirror is perhaps replaced by one that is flat. The reasons which have determined me to employ the perforated mirrors under all circumstances are the following:

1. The eye of the observer can always see with facility by looking through the opening in the direction of the rays reflected by the illuminating mirror (Fig. 4). In this way, the same objects that are found reflected by the laryngeal mirror are always observed illuminated in the most vivid manner.

FIG. 4.

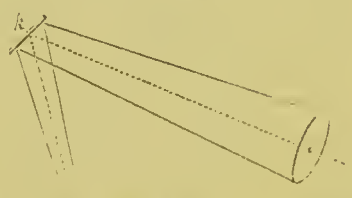

Fig. 4. *k*, Laryngeal mirror; to the right is the illuminating mirror, perforated. The continuous lines represent the outline of the luminous fasciculi which are interrupted at the laryngeal mirror *k*. The dotted line represents the visual axis of the eye placed before the mirror.

2. There is never any risk of throwing a shadow with the head upon the objects which it is desired to illuminate.

3. Lastly, the individual can be examined in almost any position that is desired.

The illuminating mirror, which is flat or concave, possesses a circular form, a diameter of three to four inches, and a focal distance of eight to twelve inches; it is attached to a stem with two branches, and remains moveable upon its horizontal axis; it can be arranged at a convenient inclination by the aid of a screw which is placed upon the longer pivot. The spot perforated, or not silvered over in the mirror, has a diameter of one fifth of an inch, and is placed at or a little to the outer side of the centre, but always in the horizontal axis.

As in ophthalmoscopic observations, the mirror is placed in a convenient position before the eye; it is there maintained, either by the aid of the hand which holds the stem, or, to permit of both hands being free, by the aid of a handle introduced between the teeth, or, again, by means of the frontal bandage of Kramer (Fig. 5),

FIG. 5.

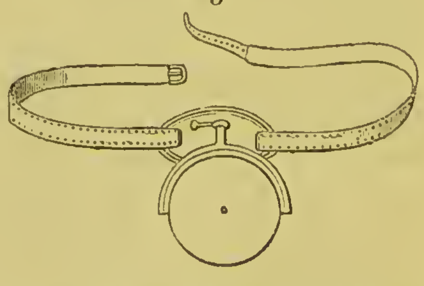

Fig. 5. Frontal bandage of Kramer, with its pinion, to which the stem of the mirror is attached with the assistance of a free joint. The pinion is placed above the right or left eye.

which I have applied to this use, or, lastly, according to the custom of Semeleder and Stellwag, by arranging it in a similar manner to that of a pair of spectacles.

The handle introduced into the mouth is a plate three and a quarter inches long, a width a little over three quarters of an inch, and a thickness of three sixteenths of an inch; the anterior extremity is mounted in brass, and carries laterally a small button perforated horizontally and vertically; the stem of the mirror can be fixed in the holes of this button by means of a small screw. If we place this stem in the vertical hole, and sustain the buccal handle between the molar teeth, with this button directed outwards from the angle of the mouth, the mirror is found to be placed vertically before the eye (see Frontispiece, Plate I). If, on the contrary, the stem of the mirror is inserted into the horizontal opening, the buccal handle serves as a simple point of attachment for the stem of the mirror, and perhaps can be maintained and directed with the hand.

Individuals who are in the habit of making use of ophthalmoscopic instruments adopt, without difficulty, one or other of these arrangements. To the short-sighted, I would recommend the spectacles invented by Semeleder and Stellwag; the suitable glass can be placed in the empty frame.

As for myself, I give the preference to the buccal handle which I invented, because the direction and manipulation of the mirror becomes remarkably easy, and also on account of its simplicity and convenient size; in fact, it is sufficient to close the teeth firmly to hold the handle, and to arrange the screws in the button and in the axis of the mirror, to render the last immoveable.

As soon as the mirror is placed before the eye, it suffices, in order to direct the light, to execute certain slight movements with the head.

We obtain the most vivid light, in directing the summit of the luminous cone upon the objects to be examined; we soon learn to place the illuminating mirror at a suitable distance from the source of light and from the laryngeal mirror.

I need scarcely add that we ought to avoid placing the parts in the mouth within the focus of the mirror, when we make use of the light of the sun.

With a wish, also, to employ artificial light, Dr. Turek used at first globes of glass employed by shoemakers ('Bib.,' No. 10;

Stoerk, 'Bib.,' No. 18). It appears, however, that latterly he has returned to my method of illuminating by means of the ophthalmoscope; he describes, at the same time, a foot of the illuminating mirror, articulating like an arm ('Bib.,' No. 19).

There remains yet another method of illuminating the different parts of the larynx. I have called it *illumination by transparency* ('Bib.,' No. 2); in fact, the larynx and the trachea have, as I pointed out, in certain spots, a certain degree of transparency, the same as the tissues which they cover. In delicate persons, we perceive the glottis illuminated by a reddish light, when the sun falls upon the external surface of the neck. But when we concentrate the solar light by the aid of a concave mirror, or by that of a lens, upon particular spots about the neck, some one or other part of the larynx becomes transparent, like the finger placed before a flame. This transparency, of a beautiful incandescent red, is at times sufficient to permit of laryngoscopic examination. The concentrated light from gas sometimes produces, but less distinctly, the same effect. It would be possible to employ my proceeding, which has been likewise confirmed by Dr. Gerhardt ('Bib.,' No. 15), to the examination of the thickness of the vocal cords, varying according to physiological or pathological circumstances, and above all to the correct appreciation of the seat and depth of the alterations in the trachea.

Section III.—*Of Autolaryngoscopy.*

Garcia says that, "If the observer experiments on himself, he ought, by means of a second mirror, to receive the rays of the sun, and direct them on the mirror which is placed against the uvula."

Since my first attempts made with artificial light, I have adopted a method somewhat different. In fact, I place as close as possible to the mouth, widely open, the flame of a lamp, previously removing the globe, and in this manner I have illuminated the pharynx.

I then hold horizontally, between the eyes and the flame, an oblong quadrangular mirror with one hand, of sufficient size to protect the eyes from the light and to give an image of the illuminated pharynx. We prevent the flat mirror from becoming tarnished, by heating it above the flame of the illuminating lamp, as we are in the habit of doing with the laryngeal mirror.

Fig. 6.

Fig. 6. The right hand holding the laryngeal mirror; the left the flat mirror, represented here only in profile.

Fig. 6 represents this arrangement. The light of the lamp passes beneath the inferior border of the flat mirror, and passing between the two hands, it reaches the laryngeal mirror through the mouth.

The employment of a concave perforated mirror as a means of illumination in laryngoscopy compelled me at first to make use of the foot-piece of the ophthalmoscope of Ruete; afterwards I contrived a particular apparatus (Fig. 7, p. 19), intended for auto-laryngoscopy and demonstration.[1]

Fig. 8, at page 21, serves to show the mode of using this apparatus. The rays of light, which are furnished by a strong lamp, fall upon the concave illuminating mirror, whence they are concen-

[1] Messrs. Weiss and Son, of London, provide not only Professor Czermak's apparatus for autolaryngoscopy and demonstration, with two laryngeal mirrors in glass, in a mahogany case (see Fig. 7, page 19); but also, for medical use, the illuminating mirror, with a series of laryngoscopes in glass or steel, of different sizes, in a morocco case.

trated in the back part of the mouth of the person who is examining himself. (See the continuous lines ——.)

The laryngoscope, placed against the uvula, throws a luminous cone below, upon the parts which we ought to examine, and gives their reflected image, after having illuminated them.

The person who examines himself looks into the quadrangular mirror; the stranger, whom we wish to render a witness of the researches made upon ourselves, places his eye in front of the central opening of the illuminating mirror. In examining the course of the rays indicated by the dotted lines, we can readily convince ourselves that the two observers never see

Fig. 7.

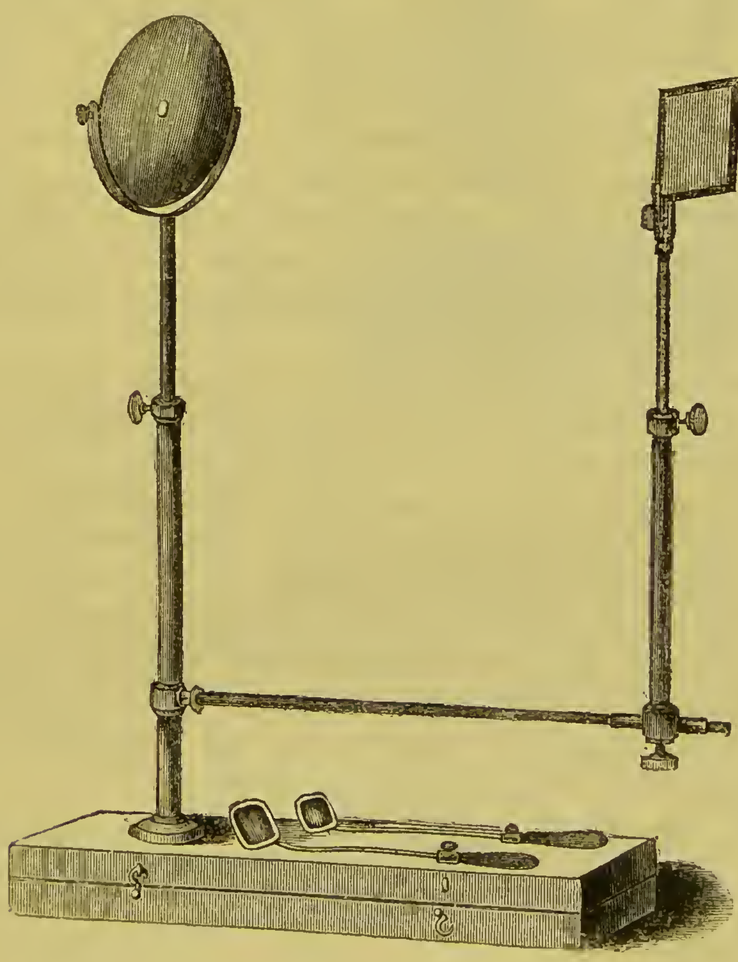

Fig. 7. Apparatus for autolaryngoscopy and demonstration. The case serves as a means of support. The large, round, perforated mirror is the illuminating concave reflector; the small square one, which is attached to the stem by a hinge, is flat. On the case are seen lying two laryngoscopes of glass.

absolutely the same image in the laryngeal mirror, because their visual axes do not form the same angles with the reflecting surface of that mirror.

This angle is more obtuse to the person who observes himself, than to the other by whom he is observed; the former will always see more of the parts situated behind, the latter those placed before. Thus, for example, in Fig. 8, the simple dots represent the rays proceeding from the trachea, and directed towards the central opening of the illuminating mirror to the eye of the stranger, after having been reflected upon the laryngeal mirror. The rays, on the other hand, indicated by a series of dots and lines (—.—.—.—.) come from the posterior part of the larynx, and reach the eye of the person who is examining himself, after undergoing a double reflection, one upon the square mirror, and the other upon the laryngeal mirror. Nevertheless, there remains, most assuredly, numerous spots which the two observers can examine simultaneously.

Other observers can place themselves either behind the individual who examines himself, and looking into the small square mirror, or at the side of the illuminating mirror, on approaching the eye to the border of this latter. By the aid of my apparatus, we can simultaneously show to a certain number of persons any part of the larynx whatever.

It would not be impossible to project the image, vividly illuminated, upon a screen, and then we could give demonstrations before a numerous audience.

A photographer whom I consulted thought it possible to fix the images that I demonstrated upon myself. The lighted chamber of Wheatstone placed behind the opening of the concave mirror permits us to take very exact sketches and measurements.

I can recommend this autolaryngoscopic apparatus to whoever is desirous of making observations upon himself without the assistance of others, or to show to others the various parts of the larynx and the movements which they perform.

Many persons will remember the clearness and beauty of the images which were obtained by the aid of this apparatus, whilst making demonstrations upon myself at Vienna, Pesth, Leipsic, Berlin, and Breslau.[1]

[1] We might refer also to the numerous demonstrations made by Dr. Czermak at Paris and in London, in the hospitals and elsewhere.—*Translator.*

Although we readily comprehend the style and office of this apparatus, it is necessary, however, to possess a certain skill for

FIG. 8.

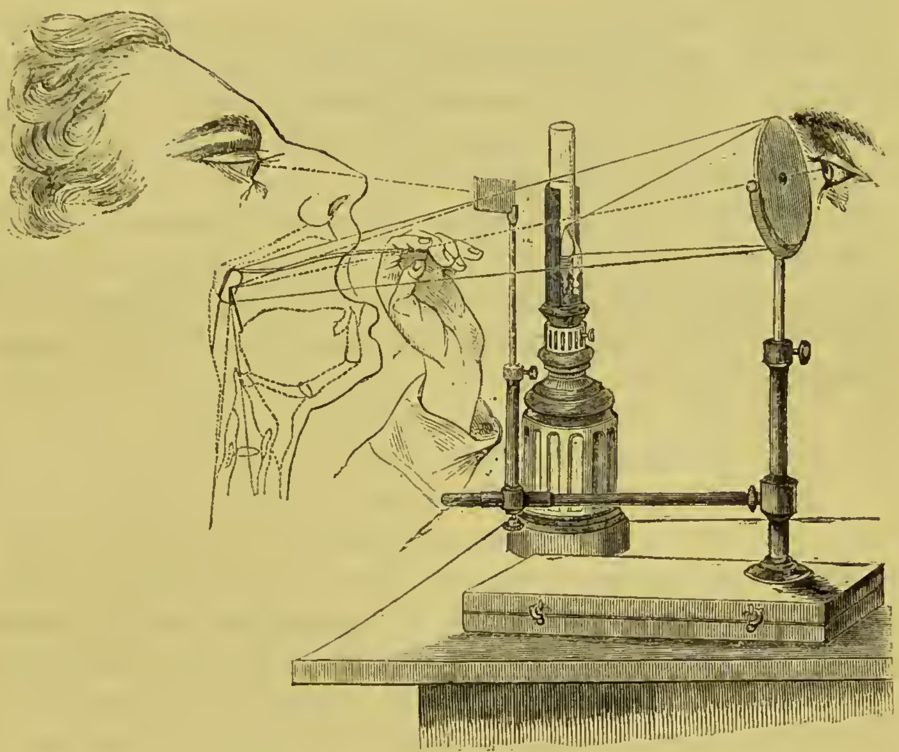

Fig. 8. Autolaryngoscopic observation and demonstration. In order not to confuse the sketch the lamp is represented farther from the face than it ought to be in reality. A demi-cylindrical reflector is attached to the lamp.

its employment. I shall confine myself in this place to a few short observations. If it is desired to make use of sunlight, we must place ourselves a little obliquely, with the back turned towards the sun. If, on the contrary, we employ artificial light, we place the lamp in such a manner that the flame is elevated to the height of the mouth, to the left of the head; the eyes are protected by means of a screen or of a reflector, and the luminous fasciculi are conducted beneath the flat mirror into the back part of the mouth. Generally, a small part of these fasciculi are found to be kept out by the back of the flat and square mirror.

The mouth being kept well open, and the tongue flat, we ourselves introduce the laryngeal mirror, warmed, gently advancing it forwards into the back part, and trying to pass beneath the velum

palati. Then the back of the mirror is placed against the soft palate and uvula, which at the same time are gently raised. The stem of the laryngoscope is found to be at that instant in one of the angles of the mouth.

The image perceived informs the observer of the changes which he ought to produce in the inclination and position of the laryngoscope, by the aid of the movements imparted to the handle, and of the disposition of the organs of the mouth and pharynx, so that the desired end can be attained.

When a demonstration is made, we must remember, relatively to the inclination of the laryngoscope, what has been said regarding the connection that exists between the plane of vision of the person who is examining himself and that of the strange observer (see p. 20).

Section IV.—*Of the Examination made upon Others.*

I believe it is superfluous and wholly unnecessary to descant upon the difficulties which we encounter in various individuals, upon the means of overcoming them, and upon the indications already given concerning the examination of certain parts of the larynx. I believe this, because, if we master the principle of the method of Liston and Garcia, if we sufficiently know the laws of catoptrics and the topography of the parts which we are anxious to examine, we shall readily understand the practical application of the general directions which have been given (Chapter II), whilst the most elaborate descriptions would never serve to instruct an observer, for the necessary aptitude can only be acquired by much study, practice, and the persevering employment of the instruments.

I shall limit myself here to add only certain short observations, which throughout concern the employment of light by means of perforated mirrors, in the following words of Garcia: "The method . . . . . consists in placing a little mirror, fixed on a long handle suitably bent, in the throat of the person experimented on, against the soft palate and uvula."

The group figured in Plate I (Frontispiece), I had engraved after a photograph; this design affords the example of an examination made with the light of the sun, which comes from above, behind and to the right of the person being examined. If artificial

light is made use of, the lamp is placed at the corner of a table, to the right of the patient, in such a manner that the flame is found to be at the same height as the mouth—a little behind the right half of his face.

The person examined places his hands upon his knees, the upper part of the body is advanced forwards, the neck bent onward, the nape slightly inclined backwards, the mouth widely open, the tongue flattened and held a little without.

The observer is seated in front of the person to be examined; he places in his mouth the handle which supports the illuminating mirror, and looks through the central opening; the laryngeal mirror, introduced into the back part of the mouth with the right hand, is illuminated by the light which is projected from the illuminating mirror; the left hand can be placed upon the shoulder of the person examined, and steadies the chin and the nape, or in holding a tongue depressor, which we can often trust to the patient himself.

In the first place, the illumination of the back part of the mouth and the mutual position are regulated; then the laryngoscope is heated, and its temperature regulated by the touch. After these preliminaries are gone through, we request the patient to open the mouth wide, and alternately to inspire deeply and to pronounce the sound *ah*; during this, we endeavour to place the back of the laryngoscope against the uvula and the velum palati, to sustain these parts a little, and to give to the mirror a convenient inclination; at times it is impossible to avoid touching the posterior wall of the pharynx; the examination is directed by the image we thus obtain (see p. 22).

In this way we can commence each laryngoscopic examination. Practice and reflection will bring each observer to comprehend the modifications to which he ought to submit this proceeding, according to the special circumstance; whether, for instance, he is in some degree to advance or to withdraw the laryngoscope, to bend it, to lower or to elevate it, to change the position and attitude of the individual undergoing examination, raise his chair, &c.

We would advise those who wish to learn the laryngoscopic method, and who already possess a knowledge of anatomy and physics, to obtain a preliminary acquaintance with it, by exercising with the laryngoscope, according to the recommendation of Dr. Turck ('Bib.,' No. 5), upon the dead body or upon the larynx which

has been removed. Stoerk ('Bib.,' No. 18) proposes to those physicians who have not a dead body at their disposal, to study the larynx of dogs, of pigs, or of sheep. But the best preparation, according to my experience, is autoscopy. (See the preceding section.)

As soon as a certain degree of expertness is possessed in the management of the instrument, we attain our aim in *many cases rapidly*, and with a surprising *facility*, because the laryngoscope, if well introduced, is borne, in the greater number of adults, much more easily than has been supposed, and this is proved by experiments repeated a hundred times.

Sometimes, however, we do not succeed more or less completely until after numerous trials.

There are a certain number of individuals in whom the organs of the throat are very irritable, abnormally proportioned, or placed in an abnormal position, &c.; on them the first examination does not give any definite results, or at least they are not satisfactory. In such cases we can, if it is not wished to make use of narcotics, by *methodical practice*, successively remove each difficulty, and a few days, as I have ascertained, are sufficient to obtain a good result; thus we can advise the opening of the mouth and to keep it widely open, to flatten and to advance the tongue, to render the back part of the mouth less sensitive, frequently placing it in contact with foreign bodies, &c.[1] This practice will not be found void of utility, even when the first examination has already given satisfactory results, either because we may have the intention of repeating the examination more exactly and more conveniently, or because we may wish to render the performance of an operation possible, or of any manipulative treatment whatsoever (see Section VI, page 34).

[1] We read in the 'Gazette des Hôpitaux' of the 21st of April, 1860: "A method has been proposed of facilitating this examination, which to us seems to possess sufficient merit to entitle it to consideration. Recent investigations have shown that the bromide of potassium possesses the property of producing an insensibility of the pharyngeal region. This singular and special anæsthetic property seems to have been used also with advantage, principally by Professor Gosselin, for an operation of staphyloraphy.

"We would ask if the same means could not be made use of, to render the palato-pharyngeal region momentarily insensible to the contact of instruments, so as to facilitate the examination of the larynx."—*Note of the German Translator.*

This preparation of the patient necessarily retards the examination, but this is not an inconvenience in chronic cases. We have likewise recommended the practice of autolaryngoscopy with advantage to intelligent patients, especially in chronic cases, where repeated observations are necessary.

But all these proceedings become impossible under the unfavorable circumstances previously enumerated, in acute affections, where we cannot afford the time necessary for the preparation of the patient.

Nevertheless, this inconvenience diminishes nothing of the incontestable value and practical importance of the laryngoscopic method, in the eyes of persons who know and are able to appreciate it. Every method has its limits.

Section V.—*Of Rhinoscopy.*

I have already pointed out, in my first notice of the laryngoscope ('Bib.,' No. 1), that the principle of the laryngoscopic method could be equally applied to the inspection of the posterior wall of the velum palati, of the superior parts of the pharynx (*pharyngo-nasal vault*), and of the posterior orifices of the nasal fossæ, &c.

If I now again dwell upon this subject, I do so for the purpose of citing a novel instance of the varied application of the principle of Liston and Garcia; I do so likewise to invite physicians, who specially study these regions, to apply and to improve my method, which I have called rhinoscopy, as it *actually* allows us to see these parts; as well as to explain the experiments which I have performed upon myself, and some more recent observations which I have made upon the dead and upon patients.

My first attempts date from the winter of 1857-8, and were made upon myself. In the first place, I arranged my autolaryngoscopic apparatus as for the inspection of the larynx; then I elevated the velum palati above and in front, voluntarily relaxed, by the aid of a blunt and flat hook. A small laryngeal mirror, previously warmed, was subsequently introduced under the velum, having its surface turned obliquely upwards, in such a manner as to illuminate the pharyngo-nasal cavity, and to send back the image of these parts.

By frequently practising this, it was possible for me, at that time, to see very distinctly upon myself a portion of the nasal fossæ, the septum, the turbinated bones, the posterior surface of the velum, &c. Actually I am sufficiently experienced to show these parts, as well as the orifices of the Eustachean tubes, (see Fig. 9).

Fig. 9.

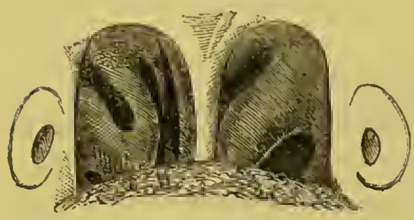

Fig. 9. Septum, posterior orifices of the nasal fossæ, turbinated bones, and orifices of the Eustachian tubes. The posterior arched surface of the velum covers the inferior part of the nasal cavity.

When I examine other persons[1] I make them depress their tongue themselves, by means of a tongue depressor; the velum then becomes readily accessible, and when wanted, I elevate it and draw it forwards with the hook, so as to facilitate the introduction of the mirror.

In order not to occupy the two hands simultaneously, of which one directs the laryngoscope, whilst the other holds the hook, I caused to be made, at the commencement of my rhinoscopic studies, a small instrument, which might serve at the same time the purposes of a hook and a mirror, and which would necessitate the employment of one hand only.

It was a metallic tube, bent at a right angle, and therefore composed of two arms, of which one is long, whilst the other—the vertical—is short; an oval, steel mirror is attached at an angle of 45 degrees, at the bent part, between the two arms. The light enters through the long arm; the short arm is obliquely cut at its extremity, to be introduced behind the velum into the pharyngo-nasal cavity.

But I soon convinced myself of the necessity of substituting for the major part of the long arm a half tube, in order to have more of the light, and a more easy inspection of the small oval mirror. It seemed to me equally advantageous to replace the short arm by a vertical plate of metal, which could be moved and removed by means of a ring; the introduction of the instrument was thus facilitated.

This simple and, it may be said, primitive instrument is represented in Fig. 10; I have often employed it with success upon myself firstly, and afterwards upon patients.

The position and inclination of the apparatus are the determining cause, by means of which some parts of the pharyngo-nasal cavity

[1] See the note p. 28. These demonstrations were made at Paris and in London, by Dr. Czermak, upon himself and numerous patients.—*Translator.*

and of the nasal fossæ, rather than others, are found illuminated and reflected by the mirror.

In July, 1859, I performed some rhinoscopic experiments upon a dead body, for the purpose of more readily exposing those regions, till then inaccessible to view, and for the better appreciation of the bearing of my new method of exploration. These again convinced me that in this way we can expose to view the posterior wall of the velum, the walls of the pharyngo-nasal cavity up to the sphenoid bone, the orifices of the Eustachian tubes, and the extent of the posterior orifices of the nasal cavity as well as the turbinated bones. I did not perceive the floor of the nasal cavity, nor of the inferior meatus; but we might succeed in doing so by using double mirrors, of which the superior might reflect its objects into the inferior.

Fig. 10.

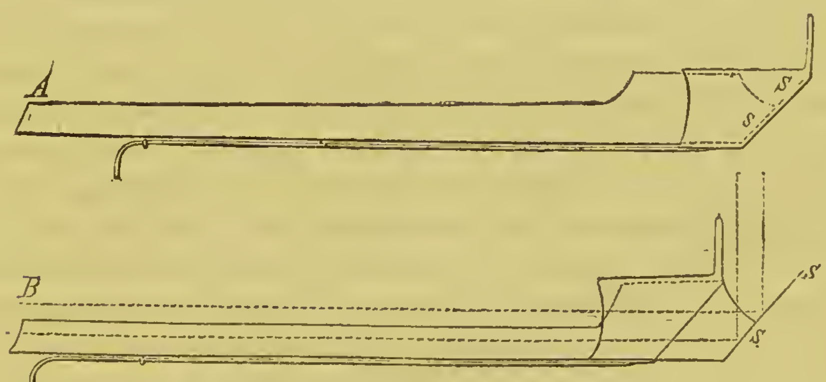

Fig. 10. Model of an instrument intended for rhinoscopy. A represents the instrument before, and B after, introduction. In this latter figure the ring with the plate of metal is drawn forwards, and the mirror *s s* is uncovered. The dots indicate the course of the rays.

I should add that I have been able, in concentrating the light upon the nose of the dead body, to make an examination of the parts now become of an incandescent red by reason of their transparency, as occurs in the larynx under similar circumstances (see p. 17).

The difficulties of rhinoscopy, unless favoured, for example, by insensibility, by fissure, or partial deficiency of the velum, are much graver than those of laryngoscopy. Nevertheless, after the experiments which I have made, there is no doubt that perseverance and practice can overcome these obstacles[1] in the one case as well as in the other.

[1] By the aid of rhinoscopy I showed for the first time, at Pesth, 29th July,

In order to show how much rhinoscopy justifies the hopes which I have formed of its general application in medicine, and in order also to incite others to employ and to improve it, I shall here relate the first pathological observation made in the pharyngo-nasal cavity by means of the rhinoscopic method upon a patient afflicted with an alteration of his hearing, and attended by my friend, Dr. Hirschler, of Pesth. I am indebted to his kindness for the following account of it:

"F. S—, a student, has already suffered, since the age of four years, from an otorrhœa of both ears, which, up to the age of fifteen, was renewed almost every year, or at least every two years. (His young brother, who is actually living at Pesth, has likewise become a little deaf in consequence of an otorrhœa; and another brother, still quite young, is also afflicted with otorrhœa.)

"It is now two years and a half since I saw this patient for the first time, who attributed the violent pains in his ears to a jump with his head forward from a jumping-board, at the swimming school. The symptoms present were those of a dermoid inflammation of the external auditory meatus throughout its entire extent, accompanied by an affection of the tympanum; but the two diseases rapidly disappeared by the seclusion of the patient for a certain time to his room, by local bleeding, and by an antiphlogistic regimen. Six weeks after, the patient suffered from a relapse; the disease had become more acute, and was likewise accompanied by severe pain and buzzing of the ears; it became necessary, nevertheless, to apply a blister, sprinkled with tartar emetic, behind the ear. By this treatment the morbid symptoms quickly disappeared, and were dispelled by degrees; but the artificial ulceration which had been produced over the mastoid process was slowly cured, and left a cicatrix. Since that time, the patient had no other reason to complain of his ear.

"A short time back, as I was informed by his aunt, this young man was for a long period positively a little deaf; I examined his ear in the month of February last, and observed the following phenomena: the transmission of sound by the bones of the head took

1859, to a number of persons, the various parts of the pharyngo-nasal cavity, and the extremity of a tube introduced into the Eustachian tube through the nose; this experiment was made upon an individual affected with an oblong fissure of the velum palati.

place well on both sides, when the meatus was hermetically closed; but when the auricular passage was open, the sound was clearly conducted on the right side to a distance of four inches and a half, and on the left side of but two and a half inches only. Examined with a speculum, the tympanum appeared to be almost the same on both sides; on allowing for a difference in the transparency, altogether insignificant, as it was observed besides in the right as well as the left ear, the state of the tympanum was quite normal. The uvula seemed shorter than ordinary, and slightly inclined towards the right side; the tonsils presented a small number of swollen follicles, perhaps more numerous on the left than the right side. The posterior wall of the pharynx was slightly tumified, and we could observe upon it streaks of frothy mucus. On submitting the patient to the experiment of Valsalva, he felt on the right side a strong current of air to exercise a pressure upon the cavity of the tympanum, and to distend it; whilst on the left side, the introduction of a current of air produced no sensation at first, and only did so afterwards at intervals of a few seconds, after repeated trials, and on increasing the force: which indicated, that on this side the air did not enter into the cavity of the tympanum but by means of great efforts; but even thus it only penetrated in the smallest quantity, and in consequence, the distension of the tympanum was scarcely perceptible.

"This important symptom was entirely confirmed by means of a catheter and a stream of air. The catheter was introduced into the middle part of the inferior nasal meatus of the right side, but not without difficulty, and by means of slight torsion outwards; but when it was introduced into the pharyngeal orifice of the Eustachian tube, and a stream of air made use of, auscultation revealed a regular emission of a large column of air, without crepitation or rhonchus. On the left side, the catheter passed readily through the nose; but auscultation allowed the ear to recognise during the application of the stream of air, first, an acute sound, sharp, of a high tone, and resembling a whistle; and secondly, a crepitation and a râle in the neighbourhood of the tympanum, as well as at a great distance from that organ."

Here terminates Dr. Hirschler's account.

It was about the middle of the month of February last, that I made the acquaintance of the patient, and as I then saw that he had the willingness, and was sufficiently master of himself, I requested him to allow me to examine his pharyngo-nasal cavity.

The rhinoscopic examination was made by me in presence of Dr. Hirschler, the 22d and 25th of February and the 8th of March of the current year (1860).

Fig. 11.

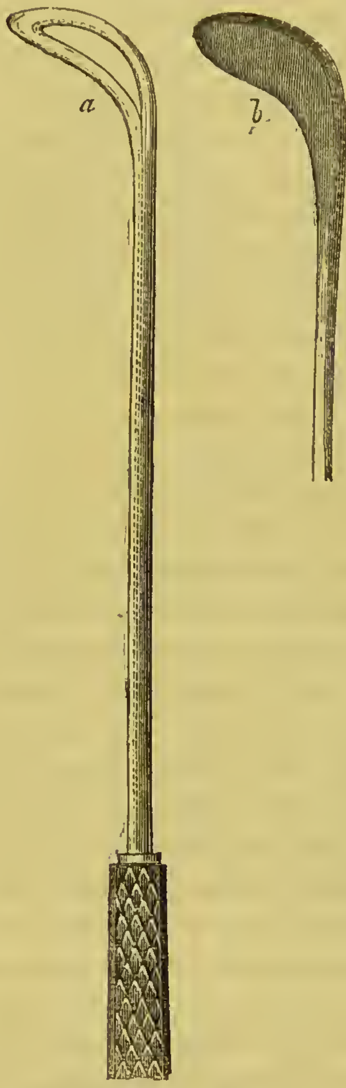

Fig. 11. Spatula in the form of a hook, intended to elevate the velum palati; *a* with an opening, *b* without one.

To perform the rhinoscopic examination of the pharyngo-nasal cavity, I took the same steps as for the laryngoscopic (see Chapter III, Section IV). I depressed the tongue by means of a spatula, and I slid behind the velum palati, which in this case was sufficiently elevated, a small laryngoscope previously warmed, of which the reflecting surface was turned above and in front. As the velum palati, left free, was sufficiently distant from the posterior wall of the pharynx, and as the uvula was remarkably small, the patient at the same time remaining tranquil, we proceeded to examine the pharyngo-nasal cavity, without any other precaution. By this inspection, I soon recognised, in a perfectly normal condition, the posterior orifice of the Eustachian tube of the right side; but on the left, I perceived a morbid state of the same region. A silver catheter, introduced into the Eustachian tube of the right side, could be observed very clearly, when made to advance and pass through the orifice. On the left side, on the contrary, in consequence of the pathological modifications which were present, we could not perceive the catheter introduced into the Eustachian tube, no more than the orifice itself.

In order to examine the affected part with greater liberty, and on a more extensive scale, I requested the patient himself to depress the base of his tongue with a rectangularly refracted spatula; and with my left hand I elevated the velum palati in front and above, by means of a flat hook (Fig. 11) with a long stem, assuming the form

of a spatula, of which the flat part contained an opening. The application of this hook did not cause the slightest inconvenience in the present instance, and after regulating the inclination and position of the mirror which I had introduced into the pharynx with my right hand, I was enabled to complete, by degrees, a satisfactory and clear examination of the part affected, that permitted me to make a representation of the state of it, as shown in the annexed figure (Fig. 12).

We observe, in Fig. 12, the posterior orifices of the nasal fossæ separated by the septum, the posterior surface of the velum palati with the uvula slightly inclined towards the right side, and the posterior orifice of the Eustachian tube of the right side.

Fig. 12.

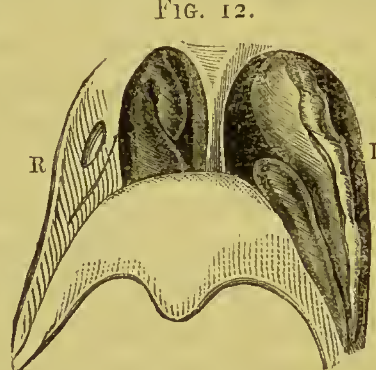

Fig. 12. Rhinoscopic view of the posterior nares, and of the velum palati. On one side (R), the orifice of the Eustachian tube is seen, which, on the other side (L) is covered by two tumours.

The right side (R), which is naturally the left side of the image reflected by the mirror, is perfectly normal, and we can perceive through the posterior orifice of the nasal fossa of the same side a portion of the upper part of the turbinated bones, and a portion of the meatus of the right side, whilst the inferior parts are hidden by the velum palati.

On the left side, on the contrary, we observe two morbid swellings of the mucous membrane developed, in front and above the pharyngo-palatine arch, which extend up to the opening of the Eustachian tube. These two tumours prevented our seeing the catheter when it was introduced, and seem to conceal almost completely the posterior orifice of the nasal fossa of the same side, which are seen only in the form of a small fissure.

These tumours, of which one part assumed the form of the comb of a cock, and of which the base constituted the lateral wall of the pharynx, were enclosed between the posterior wall of the pharynx and velum palati, which they compressed a little to the right by their superior part; this explains why the uvula was not found in the median line beneath the septum of division of the nasal fossæ.

The superior tumour, which was the larger, corresponded by its swollen extremity to the posterior border of the orifice of the

Eustachian tube; and the inferior tumour, which was much smaller, embraced the anterior and inferior limit of the same orifice. The two tumours were of a much darker colour than that of the surrounding mucous membrane, and when they were touched with a sound introduced in the course of the rhinoscopic examination, they exhibited a firmness which was well marked.

The pathological observation which has been described presented no other important or fresh results in the diagnosis of the patient's affection, which could be realised very well by means of the data of the ordinary otiatric method; but it completed the description of the disease in a satisfactory manner, and demonstrated the utility of the rhinoscopic method, and the possibility of employing it, without which we should never have arrived at a sufficiently complete description of the disease.

The preceding observations seem to prove that the rhinoscopic method succeeds sometimes readily, even when the velum palati is in a perfectly normal condition, and that the further completion of it promises to give the best results. Whilst on this subject, I may be permitted to revert to the principle which served as a basis for the construction of a demi-tube with a mirror (see Fig. 10); because this temporary instrument, or an analogous one (perhaps with a moveable mirror, or with two mirrors), ought to render possible the examination of this cavity even in cases where the application of instruments is unusually difficult, and provokes the reflex phenomena which especially appear, when the operation is done without proper discrimination.

It is only in the future, when the impulse which I have given to the subject shall have been followed for some time, that we can attempt to judge to what point the local depression of sensibility by means of cold water, of anæsthetics, narcotics, or the patients getting used to it, on the one hand, and the gradual increase in the powers of dexterity of the physician, on the other, may facilitate the use of the rhinoscopic method, if not favoured by circumstances.

I would advise those who seriously desire to occupy themselves with this subject, to commence their preliminary studies upon the dead, in order to make themselves familiar with the regions which have occupied us, and the better to judge of the relative positions of the parts concerning which our knowledge is not so perfect, and of which the image in the mirror is in no wise familiar to us. The rhinoscopic examination, as I have proved by experience, presents

no other difficulty worth mentioning, in persons among whom there is a division, insensibility, or partial deficiency of the velum.

Both before and after extracting naso-pharyngeal polypi, rhinoscopic observation appears throughout a duty to be performed, and ought, under such circumstances, to afford especially a great facility of execution in the greater number of cases.

We ought, in short, again to make a remark here, and it is, that in the regions which have occupied our attention, the eye should serve as a useful guide to the hand in the performance of a surgical operation.

As relating to the examination of the nasal cavities by the nostrils, which may be said to form a second mode of rhinoscopy, I would mention here, in conclusion, an instrument which Dr. Markusovszki, of Pesth, constructed some years ago, for this special end, and which deserves to be recommended and employed in all cases, as no other analogous instrument is generally made use of.

It is formed in such a manner as to dilate and particularly to distend the opening of the nostrils, and resembles, in its mode of construction, an ear speculum with two compartments; but its size is much greater, and the divisions are larger and flatter.

I have employed the dilator of Markusovszki several times, which I have found very easy to manipulate; and on introducing it in the ordinary manner, and making use of a perforated concave mirror, with the light of the sun or of a lamp, I could examine the nasal cavity very conveniently and very well. I especially perceived also in a manner wholly practical, the inferior turbinated bone and its free border, which passes to the extent of some twentieths of an inch, above the floor of the nasal fossæ.

In one instance wherein the nasal cavity was very capacious, I could see up to the posterior part of the pharyngo-nasal cavity.

I shall incidentally add here, that I have succeeded in introducing upon the dead, through the orifice of the nostril previously dilated, a small mirror, like that which I employ in the examination of the larynx after the operation of laryngotomy (see Chapter V, Cases 1 and 3), its reflecting surface being turned above and in front, and arranged in the space situated under the inferior turbinated bone, in such a manner as allowed of a clear view of the nasal orifice of the lachrymal canal, which was marked by a hog's bristle, that had been previously introduced.

Section VI.—*Of the Local Treatment of the Larynx.*

Thanks to the laryngeal mirror, the eye becomes the guide to the hand, and a new field of practice is granted to medical and surgical diagnosis and treatment.

Our first trials already showed the possibility of a more minute and safe local treatment, and I hope will encourage physicians to adopt that course.

In fact, I have succeeded in introducing a sound into the larynx, to ascertain the resistance which was offered by certain parts in a state of disease; besides, I have passed into it a sponge attached to a curved stem, and saturated either with glycerine or a solution of nitrate of silver; it has been even possible for me to cauterize a spot, previously made out, as for example, the arytenoid cartilages, one or other of the vocal cords, &c. (see Chapter V, Cases 1, 7, 8, 9, 10, and 13).

The proceeding which I adopt is exceedingly simple; nevertheless, it is necessary, on the part of the operator, to possess a great steadiness of the hand and much practice of its application.

It consists in holding the laryngeal mirror with one hand, and with the other to quickly introduce the sound, or the caustic holder, being guided by the image of the laryngoscope.[1]

For the performance of certain surgical operations in the larynx, I would advise throughout the galvanic cautery, which, besides possessing many other important advantages, has also that of not producing any hæmorrhage.

[1] Nevertheless, this method yet admits of some improvement. For instance, we might transform the stem of the laryngoscope into a tube, filled with a liquid or a powder, and taking such steps as shall always force the medicament to escape in a direction known beforehand; guided also by the image, we shall be certain of reaching the diseased spot. During my last visit to London I gave particular and full instructions to Messrs. Weiss and Son, Strand, for the manufacture of such an instrument.

CHAPTER IV.

PHYSIOLOGICAL OBSERVATIONS.

Before communicating my own observations made by the aid of autolaryngoscopy ('Bib.,' No. 3), I must first confirm the correctness of the results published by Garcia, in relation to the following points:

1st. In quiet breathing, the glottis remains widely open.

2d. The arytenoid cartilages perform very rapid and independent movements, when the glottis is contracted during pronunciation.

3d. During the emission of sounds from the chest, or of a shrill voice, the epiglottis assumes a different position, and is more or less distant from the arytenoid cartilages.

4th. The superior vocal cords[1] exert no influence upon the production of the voice; this is only produced, as J. Müller has proved, by the vibrations of the inferior vocal cords, distinctly to be seen, unless the sounds are too high, when the vibrations are so numerous as to become imperceptible (*"the inferior ligaments, at the bottom of the larynx, form exclusively the voice, whatever may be its register or its intensity, for they alone vibrate at the bottom of the larynx"*).[2]

I reserve for future consideration the other assertions put forth by Garcia.

Section I.—*General sketch of the Internal Arrangement of the Larynx during Respiration.*

When I examine the depth of the pharynx with my autolaryngoscope, the head being previously inclined backwards, the mouth widely open, and the tongue flattened, without protruding from the mouth, I perceive, in the almost semilunar space bounded by the

[1] The upper folds of the glottis, the superior thyro-arytenoid ligaments, or the false vocal cords.

[2] Garcia, op. cit., p. 221.

posterior wall of the pharynx and the base of the tongue, the superior part of the epiglottis free, the mucous membrane of which is more or less injected. Sometimes this membrane is pale, and, like the conjunctiva of the eye, presents only some clearly defined red vessels, beautifully ramifying over its surface.

In that position, the epiglottis touches the posterior wall of the pharynx with the superior part of its lateral borders; there remains no interval for the passage of air but below, at the two sides, and above about the middle. Through this narrow elliptical cleft, which exists between the superior border of the epiglottis and the walls of the pharynx, we can see to a greater extent forwards.

We then perceive a transverse cushion of the mucous membrane, of a reddish colour, which is closely applied to the posterior wall of the pharynx; it is slightly swollen at its two sides, and presents a small nodule more or less distinct, previous to disappearing beneath the lateral borders of the epiglottis.

The cushion is formed by the edge of the fold of the mucous membrane, sustained between the two separated arytenoid cartilages.

The œsophagus commences here, where the posterior outline of this fold encounters the wall of the pharynx; but when it is not distended by a foreign body, its walls are completely collapsed.

The anterior outline of this fold is concave in front, and forms with the superior border of the epiglottis, situated a little higher, and which is concave behind, a narrow transversely elliptical fissure, which conducts to the interior of the larynx (Plate II, fig. 1).

It is necessary to separate the epiglottis from the posterior wall of the pharynx, when we wish to render broader the field of vision in the interior of the larynx and of the trachea. We accomplish this by giving to the base of the tongue a convenient position, as, for instance, in pronouncing the sounds *ah* or *eh*. The complete emission of the vowel *e* elevates the base of the tongue to the point of entirely covering the laryngoscope; but it does not do so when we desire only to pronounce this vowel; yet the mere attempt sometimes suffices to bring about the wished-for result. In myself, the epiglottis does not change its position in the emission of an *a*, as in *all*. Fig. 2, in Plate II, represents the image given by the laryngoscope, when the epiglottis is elevated.

This arrangement corresponds to the state of tranquil respiration, and differs little from that represented in Fig. 1. The semilunar space behind the base of the tongue is enlarged, the epiglottis is

raised, and no more in contact with the posterior wall of the pharynx. The glottis is widely open, and we perceive a considerable portion of the anterior wall of the trachea. We now distinctly see a small rounded swelling, which the lateral border of the epiglottis has until then concealed from view; it is situated to the outer side of the tubercles previously described, which correspond to the cartilages of Santorini. We scarcely yet perceive an indication of the ventricles of Morgagni and the false vocal cords.

Nevertheless, it is as well to make some remarks upon the manner in which the internal parts of the larynx arrange themselves to produce the emission of the voice. In fact, as soon as we wish to utter a sound, the two arytenoid cartilages raise themselves in the fold of mucous membrane which covers them, and approach one another with surprising mobility and complete freedom. It follows that the vocal cords are drawn together by the arytenoid processes, more or less convergent, and they become more prominent and constrict the glottis. Sufficient attention and practice will make the investigator master of the movements performed by the arytenoid cartilages; he can then accelerate them, retard, or arrest them, and also examine the changes according as they take place. Fig. 3, of Plate II, represents the arrangement of the parts when the air hitherto silently expired produces by degrees the vibrations of the vocal cords. The period represented seemed to me to correspond to that of the "gentle breathing" of Purkinje.

Having thus seen the preliminary arrangements only, it is now desirable to study the manner in which the future changes for the production of sounds are brought about. It is almost impossible to study the gravest chest sounds, because the arytenoid cartilages become elevated, they rapidly approach one another almost in complete contact (Plate II, fig. 4), and bend under the border of the depressed epiglottis, in such a manner that the latter entirely conceals the interior of the larynx. We observe that, in figure 4, the posterior surfaces of the arytenoid cartilages are clearly in contact; there remains sufficient space between them and the epiglottis for the passage of air, but it is impossible to give to the laryngoscope a convenient position for the purpose of exploring the interior of the larynx; perhaps this may be accomplished by the employment of double mirrors.

Nevertheless, the examination of the larynx can be done during the emission of the less grave chest sounds, especially when they are

produced with the larynx elevated forwards and upwards, by jerks; and there is no reason for supposing that the parts behave in a manner essentially different in the emission of both the deepest and more moderate chest sounds.

Garcia has already described,[1] from direct observation, the arrangement of the parts in the interior of the larynx during phonation, and I can generally confirm the correctness of his opinions.

I have figured (Plate II, fig. 5) the condition of the interior of the larynx during the production of the most acute sounds. The examination of the parts presents scarcely any difficulties, when sounds of this character are produced. The glottis is contracted in a linear form; on each side we perceive the vocal cords, distinguished by their whitish-yellow colour from the neighbouring parts of a reddish colour; a little to the outer side is a narrow groove, which indicates the ventricles of Morgagni; further outwards again are the false or superior vocal cords. The arytenoid cartilages intimately meet, as well as their processes, in the median line, and sometimes cross over with the cartilages of Santorini; it is not unusual to observe an asymmetrical action between these parts. These cartilages raised, form, with the epiglottis pulled upwards and forwards, and also with the aryteno-epiglottic ligaments, a short and stiff tube situated above the glottis. According to the sensation which we feel during the experiment, all these parts appear to be in a state of very great tension. We perceive upon the subverted epiglottis a large part of its posterior surface, and we can distinguish a round, reddish cushion (*e, w*) very well marked, which habitually[2] conceals the point of insertion of the vocal cords, and of which we shall occupy ourselves in the next paragraph.

If, under these circumstances, the glottis is open for breathing, the arytenoid processes revolve themselves in deviating inwards or outwards, and form at the middle of the vocal cords an open or projecting angle within; in the first case, the glottis momentarily assumes the form of a lozenge (Plate II, fig. 6); but this angle is always projecting when we constrict the glottis, because the processes are then more or less convergent (Plate II, fig. 8). The posterior section of the glottis is then almost triangular, and has been

[1] Op. cit., 'Observations on the Human Voice, &c.'

[2] Dr. Czermak is now able sufficiently to elevate the epiglottis, so as to permit being seen in himself the whole of its posterior surface and the anterior point of insertion of the vocal cords.—*Translator.*

erroneously called *respiratory glottis.* If fig. 6 is compared with fig. 5, it will be observed that all the parts are relaxed; their longitudinal and transverse diameters seem to be increased. We distinctly perceive, however, that the aryteno-epiglottic ligaments are the prolongations of the fold of mucous membrane which encloses the arytenoid cartilages, and which is continued up to the lateral borders of the epiglottis.

Upon the whole we may here observe, that the larynx is surmounted by a circular fold of the mucous membrane, which is supported by seven cartilages—the epiglottis, the double cartilages of Wrisberg, of Santorini, and the arytenoid—and which form a short tube obliquely shortened from above downwards, and from before backwards, projecting into the pharynx, and of which the free border possesses a special conformation.

In fact, in myself, there can be distinguished the following peculiarities: in front, about the middle, a part sustained by the free border of the epiglottis, clearly defined, and concave behind, which loses itself below and behind in the two aryteno-epiglottic ligaments. At the posterior extremity of these last, there exists a small tubercle, which is sustained by the superior extremity of the cartilage of Wrisberg. This tubercle is succeeded by a rounded swelling, a little stronger. But this swelling, in the same manner as the whole of the border of the mucous membrane, undergoes numerous variations in different individuals. In fine, behind these swellings we perceive the tubercles bound together by the middle and posterior portion of the mucous membrane, and which correspond to the cartilages of Sartorini (fig. 6).

In quiet, deep respiration, the glottis, which has momentarily the form of a lozenge, or is divided into an anterior and a posterior part by the projection of the arytenoid processes, assumes the form of a large oblong opening (Plate II, fig. 7).

When we continue to breathe tranquilly, these parts do not change their position. The glottis is so enlarged, that I could pass a finger right into the trachea. The ordinary anatomical division of the glottis into respiratory and vocal can only be admitted for the designation of the two physiological conditions of respiration and phonation; and we can, as anatomists, speak only of one intercartilaginous glottis, and of another intermembranaceous.[1]

[1] This opinion perfectly accords with the results obtained by the physiological researches of M. Longet, who for some time has called these two portions

When the respiration becomes more or less agitated, it sometimes happens that the arytenoid cartilages execute synchronous movements, like the alæ of the nose in dyspnœa. They then slightly approach one another during each expiration; they are widely separated during each inspiration; the tubercles which correspond to the cartilages of Santorini are alternately projecting or obliterated. It is not easy to suppress suddenly these synchronous movements by the will, when they have taken place.

In examining the trachea through the open glottis, we perceive, as has been already pointed out by Garcia, the cartilaginous rings upon its anterior wall. But, on widely opening the glottis, and in straightening the trachea, I was enabled to show to Dr. Elfinger, after having fixed his attention upon this subject, the bifurcation of my trachea and the commencement of my bronchial tubes (Plate II, fig. 7, *b d*, *b s*). This surprising fact, which I had already demonstrated in April, 1858, has been afterwards proved on numerous occasions. It is impossible to observe upon one's self the commencement of the bronchi, in consequence of the position which it is necessary to give to the neck and the laryngoscope. But I saw this bifurcation for the first time in a patient, on the 25th of January, 1859 (see Chap. V, Case 6); and it was likewise perceived later by Dr. Semeleder, of Vienna, Dr. Lewin, of Berlin, and by Dr. Turek, of Vienna.[1]

Section II.—*The Mechanism of Closure of the Larynx.*

It may here be permitted me, in the first place, to recall some anatomical details already pointed out by Santorini, but since forgotten or incorrectly described, and to which my attention has first been drawn by autolaryngoscopy.

I have already stated, that I could perceive in myself, at the inferior part of the posterior wall of the epiglottis, when sufficiently elevated, a prominent reddish cushion, situated immediately above the point of insertion of the vocal cords, which it frequently

the intercartilaginous and the interligamentous glottis.—*Note of the German Translator.*

[1] In Paris, I exhibited this upon myself, to Doctors Bouvier, Desormeaux, Mandl, and some of the house-surgeons of the Hospital for Sick Children.

prevents from being seen, and which it seems even to touch in the production of the most acute sounds that it has been possible for me to emit.

But this projecting cushion is not a peculiarity of my own larynx; for I have confirmed its presence, more or less developed, throughout the greater number of persons whom I have examined by the aid of the laryngoscope.[1] On examining a larynx divided along the middle line, we see that the epiglottis, starting from the point of insertion of the vocal cords, is at first convex posteriorly, then a little higher up concave, and then near the superior free border it is again convex behind.

We see this cushion in front when the larynx is open in dividing the posterior wall. The cushion begins at the point of insertion of the four vocal cords, and is enlarged a little higher up.

Soemmering gives[2] an excellent drawing of a transverse section; and Santorini describes this configuration of the epiglottis with all the correctness of the ancient anatomists:[3]

"Epiglottis igitur etsi ederæ folio a veteribus potissimum similis habita fuit, longe absimilem eam esse, quisque facta collatione intelliget. Hujus etenim ima pars acuto mucrone assurgens, acuta productaque convexitate interius prominet, qua dein sensim paulatimque latescente circa mediam interioremque partem in concavam capacitatem explicatur, quæ circa superiora reflexis exterius undequaque oris, tandem extrema reducta, et leniter circa medium cava revoluta crepido in superiora fertur. Inferior hujusce pars latum fere digitum supra glottidem, atque adeo paulo supra laryngis ventriculorum priora extrema inferiori thyroidis angulo innectitur, inde sensim producta paulum in posteriora inclinatur, atque acuta ea, ac prominente interiore parte, quæ velut in oblongam aciem componitur, ita glottidi imminet, ut pro diverso hujusce applicationis modo, diversimode spiritus e glottidis arcto erumpens diremptus, in causa potissimum est, cur ex eo primum sonorum diversitas habeatur."

"Superiora vero media ejusdem epiglottidis latior, et cava pars in posteriora ulterius inclinatur, atque circum laterales oras laxe eo

[1] See, in Chap. V, the figures which accompany Cases 10, 11, and 17.

[2] 'Icones Organorum Humanorum Gustis et Vocis' (Francfort-sur-le-Mein, 1808, pl. ii, fig. 19).

[3] 'Observationes Anatomicæ' (Lugduni Batavorum, 1739, in 4to, chap. vi, "De Larynge," sect. 10, p. 107.

loci illigatur per membranea quædam producta vincula, quæ supra arytænoidum glandulas, quas in anatomem invexit solertissimus Morgagnus, perque nostra earumdem arytænoidum capitula oblique inferius deducuntur."

The sketch given by Sontorini[1] represents the interior of the larynx, seen from above, and is remarkable for its correctness and perfect resemblance to illustrations taken from the living (Plate II, figs. 5 and 6).

We shall now occupy ourselves with the process of complete closure of the larynx. In order clearly to observe the manner in which the various parts of the larynx behave themselves in the act of closure, I took as a point of departure the disposition which allows of the most enlarged view of its interior (Plate II, fig. 6); I have subsequently produced closure by an effort. We can thus study with facility the proceeding in its most simple and pure form.

In this manner, during complete and hermetic closure, I have observed the following arrangement:

1st. The arytenoid cartilages intimately meet at their internal surfaces and processes, and they bring the edges of the vocal cords in contact.

2d. The superior vocal cords approach the inferior vocal cords, so as to obliterate the ventricles of Morgagni; at the same time they also meet in the median line.

3d. The epiglottis being lowered, and its cushion become more prominent still, it presses against the closed glottis; the contact takes place from before backwards.

All these changes occur with such rapidity, that great attention is necessary to examine them in detail.

Fig. 9, of Plate II, represents the beginning of the hermetic closure. The cushion (*e, w*) already applied over a considerable portion of the closed glottis and of the narrowed false glottis; a very slender furrow alone indicates the ventricles of Morgagni. The closure is complete in fig. 10. The cushion of the epiglottis reaches up to the fold of mucous membrane which encloses the arytenoid cartilages. A considerable portion of the epiglottis freely overlaps the larynx. In our illustration this portion of the epiglottis is

[1] 'Anatomici summi Septemdecim Tabulæ,' ed. M. Girardi. (Parme, 1775, pl. vi, fig. 2).

sufficiently raised to allow of our seeing the closure accomplished by the cushion. In fact, when this free portion of the epiglottis is depressed, we see an image analogous to that in fig. 4, with this difference, that in the one there exists a free space for the passage of air between the epiglottis and the arytenoid cartilages, whilst in the other the closure is hermetic.

These threefold occurrences in the hermetic closure of the larynx explain the resistance which the glottis successfully opposes to the pressure of the air, without a development of much force during the effort.

When I compress the air within the chest, the larynx being hermetically closed, we distinctly observe the elastic parts to arch or curve outwards, without allowing the air to escape. When we then clear the throat, as if to expectorate, a passage is made for the air, because the elastic arched parts yield and are found to give way rapidly by the violent explosion of the air. We perceive the epiglottis, which is applied with its cushion upon the glottis, to become agitated by the distinct shocks.

The epiglottis is not passively depressed under these circumstances, for example, by the base of the tongue, but this depression is actually caused by the proper muscles of the epiglottis themselves. We observe, with pleasure and satisfaction, that Santorini, who was the first to study the myology of the larynx with correctness, attributes, *à priori*, to these muscles the effects which agree in a surprising manner with the direct observations made upon myself.

In fact, Santorini says, in speaking of the double muscle discovered by him, and designated by the name of *par thyro-epiglottidæum minus novum*: "Is non eundem prorsus usum ac cæteri descripti epiglottidis musculi præstare videtur; qui cum in imam fere epiglottidis partem inseratur, atque adeo, vel minimum, vel difficilius deorsum vellere illam queat, præ valido ejusdem cum thyroide nexu, illius inferam partem, quam scilicet acutam interius ac prominentem supra posuimus, deducit, eamque, puto, *proprius glottidi, velut apte, eleganterque conformatum operculum, applicat quodammodo, et accommodabat.* Bini autem, de quibus diximus, ary-epiglottidæus scilicet et thyro-epiglottidæus major (hunc etenim minorem dicimus) etsiamsi cum superam epiglottidis partem deorsum ducunt, et inferam quadantenus deprimere necesse habeant, ad id tamen potissimum videntur comparati, ut mediam, atque cavam, cui

præsertim adhærescunt, partem inclinent; quum ab thyro-epiglottidæo minore *infera maxime sit adducenda.*" [1]

Previous to the foregoing, Santorini has already observed of the great thyro-epiglottic: "Sicut memoratam hujusce musculi actionem, quem epiglottidæi majoris nomine vocandum censeo, potissimum epiglottidis depressioni datam esse existimo; ita tamen et eadem contractione *laryngis ventriculorum cavitates coarctari,* et ex iis fortasse quidquam exprimi posse opinor . . ." [2]

It is certain that during deglutition the larynx undergoes complete closure, which takes place in the manner described before the commencement of the act of swallowing. So we see that M. Meyer expresses a false opinion, in asserting [3] that the larynx is closed, or rather "is covered by the epiglottis, depressed mechanically by the alimentary bolus," although he is right in stating that, after the passage of the bolus, the epiglottis is raised again by its own elasticity and that of its ligaments.

If we voluntarily contract the pharynx, as in deglutition, when the mouth is widely open, in order to permit the application of the laryngoscope, we sometimes may see that part of the epiglottis turned up, which freely overlaps the closed larynx, so that a considerable portion of its inferior surface becomes visible (see Plate II, fig. 11).

This reversed portion, and the posterior wall of the pharynx, form the boundaries of a small round opening which gives passage to the air, if we arrest the further contraction of the pharynx, and if we open the glottis. The sâme figure (Plate II, fig. 11) corresponds to the situation of the parts, when we desire to gargle them. When, on the other hand, I endeavour to continue deglutition, which, besides, never perfectly succeeds with the mouth widely open, the epiglottis disappears under the base of the tongue.[4]

[1] 'Observationes Anatomicæ' (Lugduni Batavorum, 1739, ch. vi, sect. 14, p. 112).

[2] 'Ibid.,' chap. vi, sect. 14, p. 111.

[3] 'Lehrbuch der Physiologischen Anatomie des Menschen' (Leipzig, 1856, p. 159).

[4] We have suppressed, in the French edition, the third section, "Upon the Formation of Sounds, called *true gutturals,*" which is to be found in the German edition ('Der Kehlkopfspiegel,' Leipzig, 1860, p. 53); because it possesses purely a physiological interest.

CHAPTER V.

PATHOLOGICAL OBSERVATIONS.

I SHALL commence by reproducing in this place seven cases already published elsewhere ('Bib.,' No. 9); because, independently of the observation communicated by Dr. Semcleder ('Bib.,' No. 4), and of two cases described by Dr. Stoerk ('Bib.,' No. 7), they form the *first series* of observations which have well established the practical advantages of the method of Liston and Garcia.

I would recall, besides, the observations subsequently published by Dr. Turck ('Bib.,' Nos. 12 and 13), and by Dr. Gerhardt ('Bib.,' No. 15).

CASE 1. *Complete obstruction of the larynx by two tumours springing from the mucous membrane beneath the free border of the vocal cords, the result of scrofulous infiltration, and necessitating laryngotomy.*

In the month of December, 1858, I was requested by Professor Balassa to examine a patient, of whom he had published the surgical history in the 'Gazette Hebdomadaire' of Hungary.[1] Dr. Porges kindly furnished me with the following notes of the patient:

R. Z— is a young girl, eighteen years of age, of lymphatic temperament, middling size, and robust constitution; she was born of scrofulous parents, and inherits, with her brother, aged fourteen years, the same sad complaint; whilst two sisters, much elder, are exempt from it. Enjoying perfect health up to the age of seven years, she was then seized with bronchitis, which persisted during a whole year. From that time, there occasionally appeared, in different parts of the body, glandular enlargements, which, however, disappeared in the course of a short time. Five years back, the mesenteric glands suddenly became enlarged, and this enlargement produced considerable swelling of the lower part of the belly.

[1] 'Orvosi Hetilap,' No. 42, 1858.

The two articulations of the elbow became immoveable in the course of the summer of the same year; but this symptom soon disappeared from the use of the mud baths of Buda, &c. The catamenia made their first appearance in the sixteenth year; they have continued since, although always scanty, and often irregular.

With this appearance, the whole organism has assumed a most favorable and rapid development; nevertheless, the constitutional disease was not extinct, for in the course of the spring of 1858, the submaxillary glands all of a sudden became considerably enlarged, without any appreciable cause, and in the month of March they became developed into a series of scrofulous sores. The larynx commenced to swell in the month of April; we could perceive its outline prominent, and there was but little tenderness on pressure. We observed neither redness nor swelling of the pharynx; there was no fever; the voice, habitually weak, was in a whisper. The swelling of the larynx was much diminished during the following month, and this organ become insensible to pressure, assumed its natural position and size. Nevertheless, the hoarseness did not disappear, but by degrees was transformed into aphonia.

In the month of September, the patient commenced to suffer from difficulty of breathing; both inspiration and expiration became sonorous, to such a degree that, during the night, it was transformed into loud snoring, which interfered with the sleep of her friends.

From the 24th September, the dyspnœa was so violent, that the state of the patient became critical. The larynx was now painful on pressure; sleep was impossible in consequence of the oppression; every motion interfered with the regularity of the breathing, and caused anxiety.

On the evening of the 4th October, it brought on a suffocative attack of extreme violence; this continued until three o'clock in the morning, and confirmed Dr. Porges in the opinion which he had entertained for some time, of the absolute necessity of performing laryngotomy.

In spite of the opposition of certain physicians called in consultation, the Doctor insisted upon its performance, which at last was done, on the 5th October, 1858, by Professor Balassa; the patient was almost asphyxiated upon her seat, and it was necessary to employ artificial respiration to keep her alive.

Since that period she has gone on tolerably well, but the larynx is completely closed, in such a manner that it is impossible for the patient to inspire the least quantity of air through the larynx; this was readily shown, on stopping up the external aperture of the canula through which she breathed.

It was necessary, in the first place, to know exactly the seat of the occlusion, and the pathological condition of the larynx generally, in order to assist the general treatment of the constitutional disease by local treatment of the larynx; this made a laryngoscopic examination desirable.

My first attempts to examine the larynx by means of a speculum were not successful, either because the epiglottis proved an obstacle, or that the patient could not sustain the contact of the laryngoscope beyond a certain period. In order to overcome this last difficulty, I persuaded her to place a laryngeal mirror frequently at the back part of the mouth, so as to accustom these parts to the contact of a foreign body; this introduction was made either by the patient herself or by her physician.

At the end of a fortnight the sensibility had become so blunted, that it was possible to make a laryngoscopic examination with success. It may be mentioned, that I had previously succeeded in causing the elevation of the epiglottis by a deep inspiration (although this was not obtained by air passing through the larynx, but through the canula), and by attempting to utter loud sounds, principally the word *eh!* &c. I then perceived the image represented in Fig. 13, *a*. The false vocal cords were slightly swelled and injected, but moveable without any appreciable distress; in fact, it was possible to bring them rapidly in contact one against the other, in the median line, and afterwards to separate them so as to permit the ventricles of Morgagni and the inferior vocal cords to be distinctly perceived.

FIG. 13.

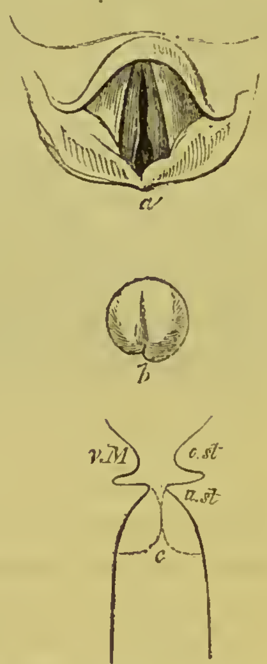

Fig. 13. *a*, superior aspect of the larynx closed by the swelling of the inferior portion of the vocal cords; *b*, this swelling seen from below upwards through the canula; *c*, diagram of the seat of this swelling; *o, s, t*, superior vocal cords; *v, M*, ventricles of Morgagni; *u, s. t*, inferior vocal cords.

These possessed almost their normal white colour, and, to my great surprise, showed a remarkable mobility. We observed particularly, during deep inspiration, or when the patient endeavoured to emit a sound, movements of the posterior portion, in which are placed the processes of the arytenoid cartilages.

Nevertheless, it was not possible to close the glottis by the approximation of the edges of the vocal cords; these were invariably separated between the processes. I perceived, between the separated borders of the glottis, a superficial furrow formed by two cushions of mucous membrane, which were oblong and of a dark colour.

The seat of the occlusion in the larynx was now recognised. It was found beneath the free edges of the true vocal cords, of which the superior lamella was normal; it was also the same in the other visible parts of the larynx, independently of slight swelling of the false vocal cords and very considerable enlargement of the arytenoid tubercles.

In order the better to understand the nature and extent of the closure of the glottis, I entertained the idea of making use of the laryngoscope according to the mode of its employment by Dr. Neudoerfer upon the dead ('Bib.,' No. 6), and which we ought to employ in all similar cases.

It was requisite to introduce a small mirror through the artificial opening of the larynx, and also to examine this organ from below upwards. With that end, I had a canula made, somewhat curved, as large as possible, and of which the superior wall had a large opening; I then introduced it into the opening made by the operation, between the cricoid and thyroid cartilages. The patient breathed with this canula as freely as with the ordinary one.

Afterwards I introduced a small metallic mirror, of which the surface was turned obliquely upwards and forwards, and placed it in such a manner that it partially occupied the opening in the superior wall of the canula, immediately beneath the inferior orifice of the larynx. The light of a lamp was now concentrated upon the laryngoscope by means of a small ophthalmoscopic mirror; the whole of the interior of the larynx, was strongly illuminated, and gave an image to the observer which was seen through the central opening in the ophthalmoscope.

Figure 14 explains the arrangement of the apparatus, and the course of the rays. We recognise, in the sketch of the neck and mouth, the representation of the fenestrated canula, which is pro-

vided with a plate of metal to fasten the straps on. We likewise see a very small mirror, the curved stem of which projects out of the canula. In front of the canula is placed the concave mirror, which concentrates the rays of the laryngoscope; the dotted line indicates the visual direction of the observer.

Fig. 14.

Fig. 14. Figure in outline of the employment of the laryngeal mirror through the fenestrated canula, after laryngotomy.

The laryngoscope, sometimes, interferes with the respiration, but it does not prevent the examination; it was warmed previous to introduction, but as it consisted of a very thin metal plate, it became rapidly cooled, and I was obliged to warm it almost every instant. I conceived a new proceeding, which obviates this inconvenience, and prevents the tarnishing of the speculum by the deposit of condensed moisture: it consists in the application of a very fine layer of a solution of gum arabic, which retains the clearness of the image for a sufficiently long time.

I could also examine very clearly and very conveniently, from below upwards, that portion of the larynx which was closed. I discovered that the round opening of the larynx was obstructed by two tumours of the mucous membrane situated beneath the free border of the vocal cords, and that they took their origin from the lateral and posterior parts. I have given a sketch of this in Figure 14, *b*; we perceive a longitudinal furrow, not very deep, at the point of contact of the two tumours. The left side of the image corresponds to the right side of the larynx, and *vice versâ*, as in all the laryngoscopic figures; but the superior border of the image corresponds to the posterior periphery of the opening of the larynx, the inferior border to the anterior periphery, or rather to the internal

border of the laryngotomie wound, where we may perceive a small vegetation.

The figure in outline (Fig. 14, *c*) represents the form of the swelling of the mucous membrane, following a section through the larynx from right to left. We recognise the folds formed by the mucous membrane at the interior of the larynx, situated one upon the other, separated by the ventricles of Morgagni (*v. M.*), and prominent towards the orifice of the larynx. The dotted lines indicate the considerable swelling of the inferior portion of the vocal cords, the result of scrofulous infiltration, whilst the internal border of the superior lamella is in a normal condition. The swelling caused the closure of the glottis.

I had recourse to the laryngoscope, in guiding my hand, to introduce probangs either from above through the glottis, or from below, in order to estimate the resistance offered by these tumours, which I found to be very strong. On introducing a probang from below upwards, and pushing it with force into the furrow, I succeeded in traversing it with the button many times. This passage was confirmed by the depth to which the instrument was driven, and by the tickling which provoked a cough, manifestly arising from touching the vocal cords, and finally by the sensation felt by the patient.

It was now proved that the passage through this place was not yet obliterated. There remained the therapeutic indication of maintaining and enlarging this by the introduction of sounds. It was this plan which Professor Balassa and myself followed in this patient, from that time.

We frequently continued the laryngoscopic examination from above and below, in order to watch the progress of the local affection. In this way we were convinced that the scrofulous infiltration of the mucous membrane, which closed the larynx beneath the free borders of the vocal cords, had extended to that portion of the latter which had hitherto remained free. At the same time it had extended downwards to the deeper parts, so that the closure occupied all the portion situated between the vocal cords and edge of the laryngotomic opening; nevertheless, the infiltrated parts were less rigid and less tense (spring of 1859).

The frequent introduction of sounds into the stricture, through the opening in the canula, produced, amongst others, the following result. After the withdrawal of the probang, we could perceive, by laryngoscopic inspection from below upwards, a canal situated

between the lips of the tumefied and infiltrated muçous membrane, which remained gaping for some time. Nevertheless, we did not succeed in forcing the least quantity of air through the larynx.

Beside the phenomenon of intelligible utterance, notwithstanding the complete dumbness of this patient, about which I made a communication to the Academy of Sciences of Vienna, presented by Professor Brucke (March 17th, 1859), I must again relate here two physiological observations which are not devoid of interest, and which I have had occasion to study also upon this patient. The first concerns the derangement of the sense of smell, caused by the closure of the larynx; the second relates to the flow of tears, which was not at all interfered with.

We know, especially from the researches of Bidder, that the direction and force of currents of air through the nose, impregnated with odorous particles, regulates the sense of smell. Ludwig states, that "currents of air impregnated with odorous particles especially produce sensations when they pass through the nose, from before backwards. Also, do we not dilate the nostrils and draw in the air rapidly and in a jerking manner, when we wish to examine the odour of a substance? It seems, then, that rapid currents of air have for their immediate result the placing in intimate contact the smelling surfaces with the odorous particles, and that the shock of the current, or the friction, occasions a pressure which favours absorption."[1]

It was not astonishing that the patient had lost the sense of smell. It is proper to remark, however, that the sense of smell was not equally destroyed, but varied according to the substances used; the patient, for instance, recognised the odour of geranium leaves which were bruised, also that of very strong perfumes, but she was insensible to the odour of violets or of hyacinths; the sense of smelling was somewhat improved, after she had learned to produce feeble currents of air through the nasal cavity, by means of sudden and energetic contraction of the pharynx and of the buccal cavity; lastly, that a current of air, which was forced by the aid of a bellows into her nose from a bouquet of violets or of hyacinths, produced a sensation which, however, was very feeble.

In what relates to the *flow of tears*, our observation proved that which is already generally known, namely, that negative pressure exercised in the nasal cavity during inspiration is no necessary con-

[1] 'Lehrbuch der Physiologie,' vol. i, p. 290.

dition of the normal flowing of tears; in fact, we have never observed the slightest inconvenience of that function in our patient. Nevertheless, we do not wish to deny that, under certain circumstances, the difference of pressure exerted by the respiratory movements can have any influence whatever upon the flow of tears.

CASE 2.—*Polypus attached to the right vocal cord, giving rise to constant hoarseness, supposed to be nervous* ('Bib.,' No. 8).

On January 2d, 1859, Dr. Hirschler brought to me a gentleman, forty years of age, belonging to the upper ranks of society, who had been attended for many years for a nervous hoarseness. This affection suddenly came on after a considerable effort (of the voice), and never wholly disappeared, but had always momentarily grown worse under the influence of violent emotion.

The laryngoscopic examination, which the patient sustained without the least difficulty, afforded me a surprising result; it showed, in fact, that the constant hoarseness was not of a nervous character, but that it was due to an accidental round and uneven production, of a dark colour, and of which its base, somewhat large, occupied a spot situated about the middle of the right vocal cord. I recognised the soft consistence of this tumour by the agitation which it underwent during the vibrations of the vocal cord, and by its flexibility when partially squeezed by the glottis. The alteration of the voice during emotion led us to suppose that changes in the size and consistence of this tumour might result from disorders in the circulation. Figure 15 represents a sketch taken immediately after the examination.

FIG. 15.

Fig. 15. Polypus attached to the right vocal cord, the real cause of a supposed nervous aphonia.

Dr. Semeleder, of Vienna, to whom I sent this patient, has since confirmed not only, in all their details, the existence of this polypus, but also, later, as it appears, the growth of the tumour.

Case 3.—*Ulcers of hard and soft palate, with considerable loss of substance, involving nearly all the uvula. Hoarseness; great swelling of the epiglottis, which is injected and ulcerated, with a mucous patch on its right side, giving it a trilobed form. Constriction of the larynx, dyspnœa, laryngotomy. Recovery, with restoration of the voice.*

For the following account I am indebted to the kindness of Dr. Koller.

S. F—, aged eleven years, a native of Baja, in Hungary, had been affected, when a year old, with a red papular eruption about the genital organs, which was only cured at the end of three months. A disease of the eyes appeared at the age of three years, and has persisted almost constantly since that period. In the summer of 1858, this patient suffered, for some weeks, from an acute disease accompanied by fever, during which the back of the nose was depressed inwards, and ulcers formed upon the hard and soft palate. At this stage, in the autumn, a very severe hoarseness was added to her disease. She presented herself for the first time for advice at the Hospital for Sick Children, at Pesth, on November 23d, 1858, and was attended during a certain time for an epiphora of the left eye, with inflammation of the lachrymal sac.

When admitted, on January 14th, 1859, into the hospital, she presented, on the whole, the following symptoms:—feeble constitution; cachectic aspect; weakness of the right eye; the back and root of the nose had sunk inwards; between the left inferior eyelid and the nose is a resisting tumour, as large as a pea, on the surface of which are seen numerous openings, the size of heads of pins, out of which flows liquid pus of a whitish colour; an ectropium of the left inferior eyelid, with the loss of nearly all the eyelashes; at the middle of the hard palate are two suppurating ulcers, situated alongside of one another, as large as the head of a pin, and, where a probe penetrates above to the depth of an inch, giving the sensation of a rough surface; considerable loss of substance in the soft palate, almost complete destruction of the uvula, with cicatricial contraction all round about; the posterior wall of the pharynx of a dirty yellow; bad odour; slight swelling of the glands at the side and nape of the neck; considerable hoarseness and noisy breathing.

I had examined the patient on the eve of her entrance into the

hospital (January 13th), and had confirmed the following condition (see Fig. 16, *a*) :—The epiglottis is enormous, extensively injected, and possesses a thickness of almost that of the finger; on the right

FIG. 16.

Fig. 16. *a*, Mucous patch on the swollen epiglottis; *b*, ulcerations formed afterwards.

side can be perceived a large mucous patch, which spreads deeply upon the posterior surface, and which is covered with thick pus, of a dirty-yellow colour. The entrance of the larynx is so constricted, that a small part of the vocal cords, slightly inflamed, alone remains visible, and the air enters and departs with a loud noise. The patient was submitted to treatment by ointments. On January 20th, I examined her for the second time, and found that not only was the right mucous patch scooped out, and its purulent layer appeared erased, but that an ulcer had also formed on the left side, small, but deep, in such a manner that the epiglottis, still swollen, presented a form altogether peculiar, of three lobes (Fig. 16, *b*). The purulent mucus seemed to arise especially from the left ulcer, between the epiglottis and the left aryteno-epiglottic ligament, rendering the breathing often troublesome, and frequently causing attacks of coughing. The entrance of the larynx was always obstructed by the swelling of the parts, although, in consequence of the retraction of the right ulcer, we could perceive a much larger portion of the true and false vocal cords of the same side.

The treatment by ointments was continued, but we did not employ local cauterization to the ulcers of the epiglottis, which, nevertheless, could have been done with certainty by the aid of the laryngoscope.

When I examined the patient over again, on January 29th, the infiltration and swelling of the epiglottis had considerably diminished; the jagged bottom of the ulcers was but slightly covered with a semi-liquid muco-pus, and possessed the reddish colour of a mucous membrane. However, the loss of substance produced by the ulcerations was considerable, and the epiglottis invariably pre-

served its trilobular form. The middle triangular lobe projected especially below and behind by its actually sharp edge.

I observed upon the right false vocal cord, always swollen, two patches of a yellowish colour, with sharp angles, and some twentieths of an inch long. The cough had diminished, but the hoarseness was persistent.

Later, we were convinced that laryngotomy was indispensably necessary, in consequence of the progress of the ulcers of the larynx and of the great swelling of the vocal cords. The operation was performed with complete success by my respected colleague, Professor Balassa, on February 25th, 1859.

I immediately took advantage of this fresh occasion to apply the laryngoscopic method from below, namely, through the fenestrated canula (see Fig. 14). It was hoped that this inspection would be more profitable than in the first case, as this was only a case of constriction, and not of complete occlusion of the larynx; and the laryngoscopic examination subsequently proved the truth of this.

I concentrated the light of the sun, by means of a small ophthalmoscopic mirror, upon an oval steel laryngoscope, placed in the fenestrated canula. There was no accidental reflection of light to interfere with the view, because the disc, which was fastened by a strap, was placed obliquely, and formed an obtuse angle with the incident rays; and because the internal surface of the canula was generally covered over with a thin layer of dried mucus. We may state that, under these circumstances, the light was truly brilliant, notwithstanding the diminutiveness in the size of the mirror; we could distinguish with surprising clearness the minutest peculiarities of the mucous membrane, in consequence of their proximity.

An ordinary lamp was found to be equally sufficient for these researches.

I have given in Figure 17, 1, a sketch of the interior of the larynx, examined a short time after the operation of laryngotomy. We examined the larynx through the open glottis, from below upwards. If we remember the situation of the laryngoscope (shown in Figure 14), we can understand why the superior part of the drawing corresponds in reality to the posterior, and the inferior to the anterior; the lateral parts reciprocally correspond to the opposite sides. The ventricles of Morgagni are not seen in this arrangement. There exists, between the free borders of the epiglottis and the right arytenoid cartilage, an opening, limited laterally, in the projected

image, by a morbid cushion and the right vocal cord; the view can extend higher up into the pharynx, through this opening.

Whilst I examined the pharynx through this opening, I requested the patient to open the mouth; and Dr. Bokai, chief physician to the hospital, convinced himself that the pharynx and the soft palate were really illuminated by the luminous rays which penetrated from below.

Fig. 17.

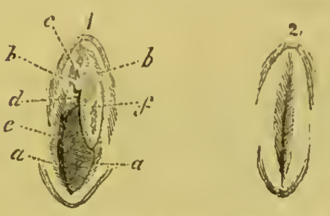

Fig. 17. 1. The larynx examined from below upwards, through the separated glottis; *a a*, borders of the vocal cords which bound the glottis; *b b*, constriction of the elastic cone, beneath the arytenoid processes; *c*, superior portion of the posterior wall of the larynx, where some patches appeared since the operation; *d*, extremity of the arytenoid cartilage of the right side; *f*, very prominent morbid cushion, situated about the height of the posterior portion of the left false vocal cord, and which is obliterated forwards and outwards; it was not distinctly visible in the examination made by the pharynx, in consequence of the position of the swollen epiglottis; *e*, inferior surface of the epiglottis, which arises obliquely backwards.
2. The constricted glottis of this patient, seen from below.

It was curious to examine the constricted glottis, from below upwards, during phonation (see Fig. 17, 2). When I requested the patient to utter a sound, which, of course, she could not accomplish, I immediately observed the borders of the glottis to approach one another, and to leave but a very narrow oblong crack, or rather a furrow, which however was not rectilinear, nor equally narrow throughout its length, because of the swelling and puffing up of the vocal cords, which still existed, although somewhat diminished. Also, in closing the canula, the voice was very feeble and hoarse.

Some weeks later, when the swelling of the mucous membrane of the larynx had considerably diminished, and when the voice had become almost natural, the narrowed glottis during phonation had its borders entire and rectilinear; we then distinctly perceived the angles which were determined by the displacements of the arytenoid processes (see Chapter IV, Section I, pages 38, 39), and which we had not seen during the swelling of the vocal cords.

The importance of the method proposed by M. Neudoerfer, which consists in an examination made through the fenestrated canula, was here incontestably proved for the second time. It allowed us to study the larynx in a precise and convenient manner from below, and rendered visible parts which, in an examination made from above, are difficult of access, or remain always concealed. We could equally in this way, as has been already observed by M. Neudoerfer, extend our view into the deepest parts of the trachea, which are not

visible from above, unless under particularly favorable circumstances.

Altogether, this proceeding appears to me of still greater importance, inasmuch as it enables the physician to control with the eye the movements of the hand, in sounding or cauterizing, or in performing any other operative measure. Experience has convinced me that, with a little practice, it is possible to manipulate nicely, and without difficulty, through the fenestrated canula, because of the proximity of the parts, and because one is but little inconvenienced by the mobility of the neighbouring organs. I believe it even possible, in spite of the sensibility of the glottis, to open an abscess, or to cut off accidental substances, which are developed within the larynx or the trachea.

I would particularly claim attention to the employment of the galvanic cautery, which, independently of any other consideration, always recommends itself by the absence of hæmorrhage.

If the laryngoscopic examination establishes with certainty (as in Case 2) the presence of an accidental production in those regions of the larynx which are difficult of access from above, and if their excision should seem necessary, it would be better to perform laryngotomy first, and then to introduce the laryngoscope in the fenestrated canula, and to operate by means of the galvanic cautery.

CASE 4.—*Cicatrices and loss of substance of the larynx, causing incurable aphonia; destruction of the epiglottis, and part of the velum palati, from old syphilitic disease.*

The mother of the patient of whom we have just spoken, E. F—, aged sixty-eight years, a native of Baja, has had aphonia for six years. She attributes this condition to a cold, occasioned, she says, by a cold drink; the aphonia suddenly supervened. She also states that she has never had any symptoms of syphilis. Nevertheless, at an examination made on January 13th, 1859, I discovered a suppurating perforation at the junction of the hard and soft palate, great loss of substance of the velum, particularly on the right side, and numerous cicatrices in the pharynx, evidently of syphilitic origin.

The larynx could be easily examined, in consequence of the insensibility of the mucous membrane of the pharynx, and also of the

loss of substance suffered by the velum palati; it exhibited very remarkable wasting, the result of laryngeal ulcerations which were now actually cicatrized (Fig. 18). The epiglottis had disappeared, and there remained only but a short stump, irregularly excavated, especially on the right side. Whatever the patient swallowed produced a choking sensation. The right superior vocal cord was considerably thickened; its surface irregular and rough. The surface of the left superior vocal cord was likewise rough, cicatrized, and had suffered besides a great loss of substance at its internal border. Consequently, on the left, we could perceive a large portion of the inferior vocal cord relatively normal, whilst on the right, the vocal cord was deeply excavated behind, and almost entirely covered by the false vocal cord. The other parts of the mucous membrane, especially the right aryteno-epiglottic ligament, presented considerable loss of substance and also numerous cicatrices. We could readily understand that this condition of the larynx did not admit of the formation of a regular glottis, capable of producing sounds. When the patient desired to emit a sound, the true and false vocal cords approached one another, and the air escaped with a hissing noise.

FIG. 18.

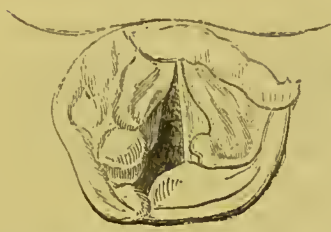

Fig. 18. Cicatrices and loss of substance of the larynx.

CASE 5.—*Retraction of the right inferior vocal cord, with swelling of the superior vocal cord of the same side, producing loss of voice, from syphilis.*

F. M—, aged forty-one years, a policeman, native of Krumau, in Bohemia, has been affected for more than ten years with various forms of secondary syphilis. Previously, in 1843, the patient lost his voice, and suffered very much with the larynx. The laryngoscopic examination which I undertook on the 22d of January, 1859, at the request of Professor Balassa, showed that the anterior membranous part of the right vocal cord was retracted in the form of a sickle, when the arytenoid process was turned inwards; the right false vocal cord was considerably prominent about the centre of the larynx, and almost entirely covered the inferior vocal cord. On the left side, the situation and structure of the vocal cords were normal. The cartilage of the epiglottis, which was recognised by its yellowish

colour through the transparent mucous membrane, presented on the right a notch or depression. Nevertheless, I do not believe that it is a pathological phenomenon, because I have likewise observed such irregularities in a state of health.

Figure 19 gives a sketch of the parts, the glottis being half open. The hoarseness is explained by the swollen superior right vocal cord, which is applied upon the anterior part of the glottis a little open, in consequence of the retraction of the inferior vocal cord. Contributing also in a great measure to form the glottis, this false vocal cord considerably restrained the production of the vibrations, which were besides already irregular in consequence of the retraction of the right vocal cord, which allowed the anterior part of the glottis to remain widely open.

FIG. 19.

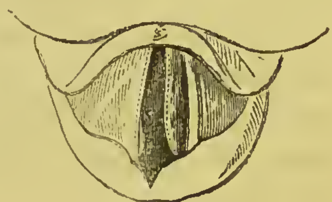

Fig. 19. Retraction of the right inferior vocal cord, with swelling of the superior vocal cord of the same side.

CASE 6.—*Partial destruction of the epiglottis, with considerable swelling of the false vocal cords; hoarseness degenerating into aphonia; congenital fissure of the velum. Inspection of the entire trachea, down to the bifurcation.*

P. P—, a clerk, was affected with a chancre in 1847; the same year, he was seized with a hoarseness, which degenerated into aphonia. His voice is better about the middle of the summer, when the weather is dry.

The application of the laryngoscope (25th January) could be made with great facility, the patient having a complete congenital fissure of the velum palati.

Fig. 20 gives a sketch of the image obtained by the laryngoscope. The glottis is half open.

The epiglottis, considerably thickened on the right side, is notched in the middle; the border and posterior surface show evident traces of loss of substance.

FIG. 20.

Fig. 20. Partial destruction of the epiglottis. Considerable swelling of the false vocal cords (superior).

The superior vocal cords are considerably relaxed and swollen, in such a manner that they touch one

another in front in the median line, even when the glottis is but half open, and that they almost entirely cover the inferior vocal cords. We can only perceive in the figure but a narrow portion of the posterior part of the border of the inferior vocal cords.

When the patient desires to utter a sound, the false vocal cords apply themselves one against the other in the median line, whilst, in the normal condition of phonation, they are found always at a distance of a few lines from one another.

This circumstance prevented the formation of regular sonorous vibrations. The air could pass only but with a hissing noise between the edges of the relaxed superior vocal cords. This abnormal state was, without doubt, in the present instance, the real cause of the aphonia.

In deep inspiration, the glottis opened itself in a normal manner, and of a normal width. The favorable circumstances which were met with in this patient, in consequence of the insensibility and conformation of the velum palati, permitted of my *examining the whole of the trachea down to the bifurcation.*

CASE 7.—*Aphonia for eight months; constriction and insufficiency of the glottis; swelling and infiltration of the mucous membrane of the arytenoid cartilages; obliteration of the ventricles of Morgagni; direct canterization of the affected parts by the aid of the laryngoscope; formation of white eschars.*

On the 26th January, I examined, at the request of Professor Balassa, M. J. R—, a merchant, aged thirty-two years. Eighteen months ago, on a very hot summer's day, he was suddenly seized with a dreadful hæmoptysis, the result of violent agitation, and after drinking a large quantity of cold water; subsequently, fits of coughing and hoarseness supervened, but the hæmoptysis did not return. The patient has had aphonia for eight months, and breathes with a sensible rattling noise. Inspiration is more or less distressing.

The laryngoscopic examination made known constriction and insufficiency of the glottis; the constriction was brought about by the swelling and considerable infiltration of the mucous membrane which encloses the arytenoid cartilages, and which necessarily limited the mobility and likewise the separation of the arytenoid cartilages and their processes; the insufficiency, on the

other hand, was produced either by the limited mechanical mobility of the parts, or, particularly on the right side, by the irregular notches on the edges of the vocal cords, which were of a grayish-white colour.

The enlargement of the glottis did not exceed some twentieths of an inch in its transverse direction (constriction), whilst occlusion without a great display of force was never hermetic (insufficiency). The patient could not compress the air in the chest unless with a great effort, and during a short space of time, because it escaped by the glottis.

I have represented, in Fig. 21, the image given by the laryngoscope, when the glottis was enlarged a little over the half of the transverse diameter, as much as it could attain. The fold of mucous membrane which encloses the arytenoid cartilages is swollen, injected, and very much stretched; it was the same with the superior vocal cords, which were likewise swollen, but relaxed. The ventricles of Morgagni were obliterated.

FIG. 21.

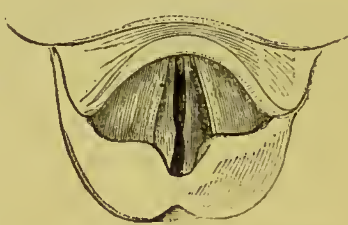

Fig. 21. Constriction and insufficiency of the glottis; swelling of the arytenoid cartilages.

Antimonials were employed by Professor Balassa at first, and afterwards quinine, when fever and night sweats supervened.

The condition of the larynx had not scarcely changed when, on February 10th, 1859, I examined the patient over again. The redness of the fold of mucous membrane of the arytenoid cartilages had almost entirely disappeared, and was replaced by a pale tint; the tension had diminished, and we thought we could see an œdematous infiltration. The superior vocal cords seemed swollen, and their anterior half were in contact at the median line.

Cauterization with nitrate of silver was indicated under these circumstances, and the following day, in the presence of Professor Balassa, I performed it by the aid of the laryngoscope, with a caustic holder, sufficiently long, and suitably curved. The quietness maintained by the patient allowed of my touching exactly the inflamed spots above the arytenoid cartilages. I introduced the laryngoscope at first with the left hand, and placed it in such a manner, that not only were the different parts of the larynx visible, but the caustic holder could be held and directed by the right hand upon the points described.

When the patient had got over a fit of coughing that was quite insignificant, which had been caused by the irritation, I convinced myself, by another laryngoscopic examination, that there existed two white eschars, clearly limited upon the parts described.

We could now consider the cauterization as having completely succeeded. There was no doubt that the precision of this operation was due only to the application of the laryngoscope. In fact, this instrument alone allowed the eye to guide the hand, and to obtain that precision in the operation which, up to the present time, has been but a lucky chance. I likewise cauterized the patient of whom I spoke in Case 6, and whose aphonia was produced by a swelling of the superior vocal cords. I repeatedly used in this case either a pencil or a small sponge dipped into a solution of nitrate of silver, and have directly operated upon the false vocal cords by means of the laryngoscope several times in presence of my colleagues. There can no longer exist the least doubt upon the possibility of operating by this proceeding. It should not even be allowed, for the future, to execute this operation otherwise than with the aid of the laryngoscope, because this method alone allows the hand to be directed, and to reach with certainty the fixed points within the larynx; whilst not unfrequently, up to the present, the epiglottis has been depressed, and its anterior surface has been cauterized, trusting to chance whether the medications would find their way into the larynx or not.

I shall recall in this place the fact, that the aphonia of the patient whose superior vocal cords were submitted to this treatment was improved, but only for a short time, so that he could utter some sounds after an aphonia existing for many years. It is necessary to remark that in this patient, and also in others, the cauterization of these very sensitive parts produced an irritation so insignificant, that it might have been believed to have failed in its purpose, if we had not seen the caustic holder penetrate into the larynx, and if the white eschars were not manifested upon the superior vocal cords.

I have chosen some rare examples from among the pathological observations which I made in 1859 by the aid of the laryngoscope, and which I owe (not practising myself) to the kindness of my respected friends in Pesth especially; I add them to the preceding. The greater part of them I have illustrated in rapid sketches, as I should give them in an oral lecture.

CASE 8.—*Hoarseness bordering on aphonia, associated with severe pain in the windpipe; application of glycerine within the larynx.*

I examined on February 5th, 1859, a young female, affected one or two days since with hoarseness bordering on aphonia, the result of a violent cold, associated with severe pains in the windpipe.

In spite of her great uneasiness and awkwardness, I was enabled to see the larynx completely, and the commencement of the trachea, in depressing and bringing forwards the tongue by means of a depressor. The whole of the laryngeal mucous membrane exhibited an active injection of the ramifications of the superficial vessels, so that they could be clearly distinguished on their yellow background. After consulting with a colleague present, and guided by the laryngoscope, I touched the larynx with a sponge dipped in glycerine.

CASE 9.—*Syphilitic ulcer of the lower lip; pain in the larynx, with redness and a small ulcer; cauterization with the solid nitrate of silver; an attack of hæmoptysis.*

A young man, M. E. S—, contracted a syphilitic ulcer on the lower lip, on kissing a female; at least, so he said.

A short time afterwards, pain was felt in the left side of the larynx. On examining him with the laryngoscope, on the 7th of June, I observed extensive redness of the mucous membrane of the left side, and at the origin of the superior vocal cord, close to the arytenoid cartilage, an ulcer of the size of a millet-seed, covered with lardaceous pus. This ulcer was cauterized with the pencil of nitrate of silver, but incompletely; it was visibly improved under the influence of general treatment. On the 20th of June, hæmoptysis suddenly took place. Some hours after this occurrence I examined the patient, and found the anterior wall of the trachea, up to the cricoid cartilage, yet covered with a thin layer of coagulated blood.

CASE 10.—*Redness and swelling of the superior vocal cords; with a circumscribed inflammation at the point of insertion of the four vocal cords.*

M. E—, aged fourteen years, has had his voice slightly masked for a long time, and he suffered from disagreeable sensations in the larynx; he was subject to frequent colds, but he was otherwise well. I examined him with the laryngoscope on the 4th of May. I introduce this case here, because I could see very easily and completely the whole of the posterior surface of the epiglottis and the points of insertion of the four vocal cords (see Fig. 22). The examination showed redness and swelling of the superior vocal cords, and their abnormal approximation during phonation. It was this which explained why the voice was masked, and even lost, during the least catarrhal affection. At the point of insertion of the four vocal cords and the epiglottis, existed a yellowish spot in the mucous membrane, intensely injected.

FIG. 22.

Fig. 22. Posterior surface of the epiglottis, and point of insertion of the four vocal cords, upon which there existed a circumscribed inflammation.

This condition could not be cured, but was improved by frequently repeated cauterization of the superior vocal cords, which were swollen.

CASE 11.—*Tubercles on the right vocal cord, and in the mucous membrane between the arytenoid cartilages; cushion of the epiglottis very prominent.*

FIG. 23.

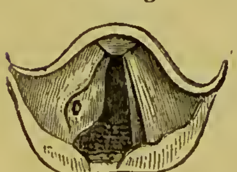

Fig. 23. Tubercles on the right vocal cord, and on the mucous membrane between the arytenoid cartilages. The cushion of the epiglottis is very prominent.

On the 26th of June, I examined a woman, A. K—, with aphonia for two years, and who had been previously affected with syphilitic ulcerations of the velum palati. Fig. 23 shows that the injected mucous membrane of the right vocal cord, and the fold which occupies the intervening space between the arytenoid cartilages were the seat of yellow tubercles. Iodine always brought about some improvement.

Case 12.—*Constant difficulty of breathing, supposed to depend upon some obstacle in the larynx; negative results afforded by the laryngoscope.*

B. S—, aged 40 years, has suffered for a long time from difficulty of breathing, constantly increasing, and at last consulted Professor Balassa, who sent her to me on the 30th of June, to be examined with the laryngoscope. In fact, the hissing respiration caused me to suppose the presence of an obstacle within the larynx, where the patient, according to her sensations, indicated the seat of her disease. I was very much surprised to find the larynx perfectly normal, the glottis widely open, and the trachea entirely free! I regretted that I could not perceive the entrance of the bronchi, although my view extended quite close to the bifurcation. The obstacle, existing neither in the larynx nor in the trachea, ought to be found at the bifurcation, or rather in the bronchi.

This case, in spite of its negative result, is extremely interesting, because it showed that the laryngoscope sometimes gives very correct counter-indications relative to the operation of tracheotomy, which, deceived by the symptoms, we might be induced to attempt.

Case 13.—*Transformation of the left superior vocal cord into a hard, rough, and ulcerated tumour, producing constriction of the glottis and dyspnœa.*

On the 7th of May, Professor Balassa sent me an old man, J. W—, who had suffered for some time from difficulty of breathing, and in whom the hoarse voice possessed a peculiar sound.

The patient believed that his disease was caused by a singular growth, which he said he could perceive himself, when he opened the mouth very wide, and protruded the tongue.

On examining the patient, I observed at the back of the mouth, behind the base of the tongue, a projecting body of a reddish colour, of the length of a third of an inch, and the width of about three quarters of an inch; but this was only the projecting epiglottis. It was clear that this arrangement would render a laryngoscopic examination very easy. It afforded the following results:

The right half of the larynx was normal; on the left side, on

the contrary, the superior vocal cord was transformed into a cushion covered with rough prominences, which overlapped the inferior vocal cord, and considerably constricted the respiratory glottis; it formed a hernia, as it were, above the aryteno-epiglottic ligaments, which it bounded (see Fig. 24). This abnormal mass was of a dark colour, and presented at its posterior extremity an ulcerated spot of the size of a lentil, covered with dirty pus. In other respects, it was of solid consistence; of this I was convinced by an examination made with a probe, sufficiently long, and suitably curved, laid upon the tumour by the aid of the laryngoscope.

Fig. 24.

Fig. 24. Transformation of the right (superior) false vocal cord into a hard, rough, and ulcerated mass.

It would have been easy in this case, according to the plan of Middeldorpf, to cut off some particles, or to destroy the whole mass, in operating with the galvanic cautery.

Case 14.—*Cauliflower excrescences upon the posterior wall of the pharynx, covering and obstructing the larynx.*

B. F—, aged 14 years, being examined with the laryngoscope, on the 26th of June, presented a cauliflower excrescence, the size of a pea, situated upon the posterior wall of the pharynx, and which covered and almost completely obstructed the entrance into the larynx. The base of the tongue, and the space situated between it and the epiglottis, were covered by a large number of smaller excrescences. On the 5th of July, Professor Balassa destroyed the pedunculated excrescence, and a portion of the others, by means of the galvanic cautery, which permitted the larynx and epiglottis to be seen quite free. The edge of the epiglottis was immoveable, and stretched transversely above the larynx; it exhibited some slight excrescences, whilst the interior of the larynx was normal.

Case 15.—*Minute condylomata upon the edges of the glottis, producing hoarseness and aphonia.*

J. B—, a young man, a clerk, was affected with a chancre in 1855; in October, 1858, a hoarseness became developed by degrees, which

was afterwards (April, 1859) transformed into complete aphonia, the result of great fatigue and cold.

An examination, made on the 21st of May, gave the following results:—injection of the mucous membrane; thickening of the vocal cords; the development of small, pointed excrescences, in the form of condylomata, upon the borders of the glottis; the presence of creamy liquid pus at the angle of insertion of the vocal cords anteriorly.

CASE 16.—*Conical excrescence upon the inferior vocal cord, with thickening, producing hoarseness; believed to be syphilitic.*

On the 20th of June, I examined an old rabbi, who came to Pesth, to consult the physicians for a severe hoarseness. I found the right inferior vocal cord thickened, and above it a conical excrescence, of the length of three to four twentieths of an inch, which was implanted near the anterior extremity of the arytenoid process.

FIG. 25.

Fig. 25. Condylomata of the larynx.

I would here remark, that this man was suspected to have communicated syphilis to several children, in practising the operation of circumcision,[1] without, as he pretended, being affected with the disease himself.

Fig. 25 represents a laryngoscopic result, of a precisely similar character, observed in a huntsman, aged 41 years, suspected of syphilis.

CASE 17.—*Accidental growth beneath the angle of insertion of the vocal cords, and above the right superior vocal cord.*

S. S—, a rabbi, robust young man, has been often affected with condylomata at the base of the tongue and pharynx; the last time about nine months ago. Hoarseness and cough frequently accompanied this condition.

[1] We know that there exists among the Jews the abominable habit, on the part of the operator, of sucking the yard of the circumcised infant.

Fig. 26.

Fig. 26. Accidental growth beneath the angle of insertion of the vocal cords, and above the right superior vocal cord. Cushion of the epiglottis very prominent.

A laryngoscopic examination, on the 14th of July, gave the following results (Fig. 26):—everything is normal on the left side; a projecting and flattened cushion of the mucous membrane exists above the right superior vocal cord; we can perceive an accidental growth, of a smooth and globular form, immediately beneath the angle of insertion of the inferior vocal cords.

Case 18.—*Polypus on the left inferior vocal cord, causing very slight hoarseness.*

M. B. W— is a robust and healthy man, with a slightly hoarse voice, which is far from indicating the very great alterations which actually existed in the larynx (Fig. 27). In fact, I found present upon the anterior half of the left inferior vocal cord within the larynx, which was otherwise normal, an accidental growth, of the size of a small pea, round, smooth, and pedunculated. It was moveable, and was found to be sometimes squeezed within the glottis by the current of air, and sometimes it projected. When the glottis was moderately open, the polypus touched the border of the right inferior vocal cord, and adjusted itself to this border, which was here depressed and swollen. The examination, repeated on the 9th and 22d of June, 7th and 14th of July, and the 14th of October, did not reveal any remarkable alteration.

Fig 27.

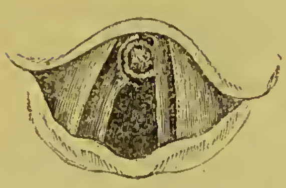

Fig. 27. Polypus of the left inferior vocal cord.

Case 19.—*Large epithelial muriform polypus, developed within and above the glottis.*

J. C—, shoemaker, 25 years of age, has been hoarse since the carnival of last year, and has had aphonia for four months; the cause of this, he thinks, is some obstacle in breathing within the

larynx. We have (Fig. 28) a very large muriform polypus, which, taking its origin upon the right inferior vocal cord, is developed within and above the glottis. The left vocal cord is normal. Nevertheless, we can perceive another similar polypus, but much smaller, arising from the posterior wall of the entrance to the larynx, corresponding to the side of the left arytenoid cartilage, and which yet more constricts the space for the passage of the air. The rough and resisting surface of these polypi is covered with a yellowish mucus.

Fig. 28.

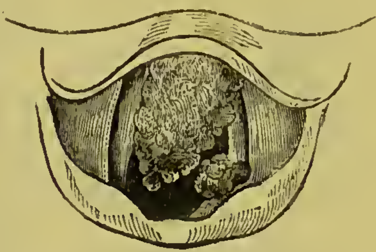

Fig. 28. Very large muriform polypus, of an epithelial character.

The last seven or eight cases, and also the second, in which, for the first time, I believe, were diagnosed on the living, with precision, the form and the seat of a pedunculated laryngeal polypus, of such a small size, may be regarded as new illustrations to add to those which have been collected by Middeldorpf.[1] They also prove, in accordance with observations made by other physicians ('Bib.,' page 10), that these accidental productions in the larynx are much more frequent than has been supposed up to the present time.

Case 20.—*Œdema of the Glottis.*

J. H—, aged 29 years, manufacturer of pipes, was discharged from the hospital a month ago, as convalescent from typhoid fever. On the 24th of October he was sent to me by Professor Balassa, because he had some difficulty of breathing. Inspiration was whistling and very difficult, expiration was easier; he suffered from uneasiness and a thirst for air.[2] The laryngoscopic examination, nevertheless, completely succeeded with the aid of a tongue depressor.

Fig. 29 illustrates the arrangement of the parts. The epiglottis, the aryteno-epiglottic ligaments, the superior and inferior vocal

[1] 'Galvano-caustik.' Breslau, 1854, chap. xi.

[2] There exists in German an expression, with which the medical French language is not as yet enriched, and which expresses in a striking manner the want which patients suffer; it is that of *lufthunger*, "hunger for air," and which we should propose to designate more exactly by "*thirst for air*."—*Note of German Translator.*

cords, were all considerably swollen by an œdematous infiltration: all these parts were very pale, with the exception of the injected epiglottis, the right side of which was the seat of a vesicle filled with fluid. The glottis was reduced, by the junction of the infiltrated and immoveable inferior vocal cords, to a small roundish triangular space, which existed between the arytenoid processes. The superior vocal cords, as well as the aryteno-epiglottic ligaments, although infiltrated and swollen, counted as nothing in the constriction of the space reserved for the passage of air. We see, then, the image of a true œdema of the glottis.

FIG. 29.

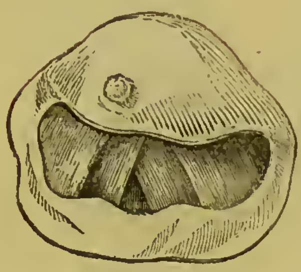

Fig. 29. Œdema of the glottis.

Laryngotomy was performed on the 29th of October by Professor Balassa. Great ease and comfort followed. An examination, made on the 23d of November, showed that the epiglottis was considerably reduced, the œdema had diminished, the superior vocal cords were moveable, and that the glottis had become enlarged into an oblong and triangular crack.

I relate this case, because it affords a very good example of undoubted œdema of the glottis, and because the state of the larynx was determined before the operation.

CONCLUSION.

This memoir, and the writings of other investigators who have followed the path which I had pointed out, may perhaps justify the "*too exaggerated*" hopes (see note, Chap. I) which I formed at the commencement of my researches, and which I previously expressed on the 27th of March, 1858 ('Bibliography,' No. 1).

Thus much is certain, that precision and accuracy of diagnosis, rendered very easy, will permit in many cases the employment of rational means of local treatment; that the pathology and treatment of laryngeal affections, and likewise of other diseases, where the principle of the laryngoscopic method can be applied, are on the point of making unexpected progress.

The physicians who, by their position and their studies, are in more favorable circumstances than a mere "theorist," will perhaps be induced by these results to persevere in their researches in all the various directions already indicated throughout this work.

VIEWS OF INTERIOR OF LARYNX.

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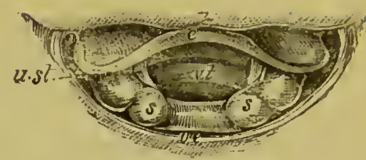

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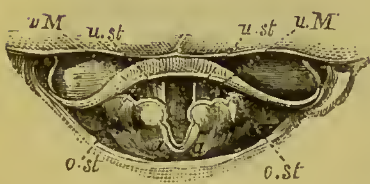

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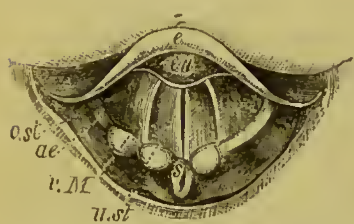

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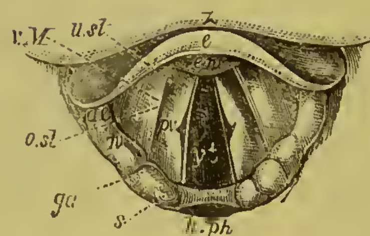

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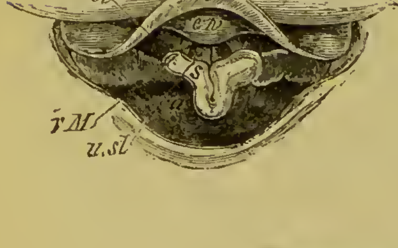

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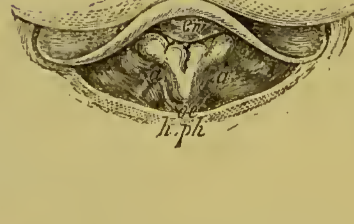

11.

EXPLANATION OF PLATE.

The figures opposite represent the arrangement of the internal parts of my own larynx, after drawings executed in March and April, 1858, by Dr. Elfinger, at Vienna, from the images demonstrated in the laryngoscope, by means of the method described in the third section of Chapter III.

The parts situated to the right of the median line of these figures, obtained by the aid of a mirror, necessarily correspond to those on the left side of the larynx, and *vice versâ.* That which is situated above in the drawing, in reality exists in front; that which is below is situated behind.

The following letters indicate in all the figures the same parts, namely:

z. Base of the tongue.

p.h. Posterior wall of the pharynx.

œ. Entrance of the œsophagus, the line of demarcation between the wall of the pharynx and the posterior surface of the larynx.

e. Epiglottis.

a. Arytenoid cartilages.

e.w. Cushion of the epiglottis.

a.e. Aryteno-epiglottic ligament.

w. Tubercle corresponding to the cartilage of Wrisberg.

s. Tubercle of the cartilages of Santorini.

g.a. Tubercle which sometimes exists between the two preceding.

p.v. Process of the arytenoid cartilages.

u.st. Inferior vocal cords.

o.st. Superior vocal cords.

v.M. Ventricles of Morgagni.

tr. Anterior wall of the trachea.

ht. Posterior wall of the larynx.

bd. Right bronchus.

bs. Left bronchus.

APPENDIX ON RHINOSCOPY.

In order to give a clearer idea of the method which I invented, and called "Rhinoscopy" (from ῥίν and σκόπέω), I think it advisable to add, in the form of an appendix to this little work on the Laryngoscope the following remarks and pathological observations.

The woodcut (Fig. 30) is meant to explain the mode of rhinoscopic investigation, already described in Chapter III, Section V.

The observer holds the large reflector, R, by a mouth-piece between his *molar* teeth, and looks through the hole in its centre. The luminous rays of the sun, or a good moderator lamp, coming from behind the person experimented on, are concentrated by means of the large reflector upon the little mirror, *k*, which is introduced with its reflecting surface turned upwards, and which throws the rays into the nasal cavity, so as to illuminate it. The image of the illuminated parts is reflected by the same little mirror into the eye of the observer.

The velum and uvula are raised and drawn gently forwards, by means of a suitably bent hook, *cr* (see Fig. 11).

In the drawing the left hand of the observer, which holds the hook, has been omitted. The observer's right hand, which supports itself on two fingers resting on the jaw, as represented in the engraving, introduces the small mirror, with its reflecting surface turned upwards, under the elevated velum, and then behind it.

Rhinoscopy renders accessible to view—through the widely open mouth and fauces—the nasal part of the pharynx and the nasal fossæ from below and behind; it must, therefore, be remembered that the canal through which we look should neither be obliterated by the tongue nor the velum, but must remain widely open. In

rhinoscopic examinations, the success depends principally on the sufficiency of the distance between the soft palate and posterior wall of the pharynx.

FIG. 30.

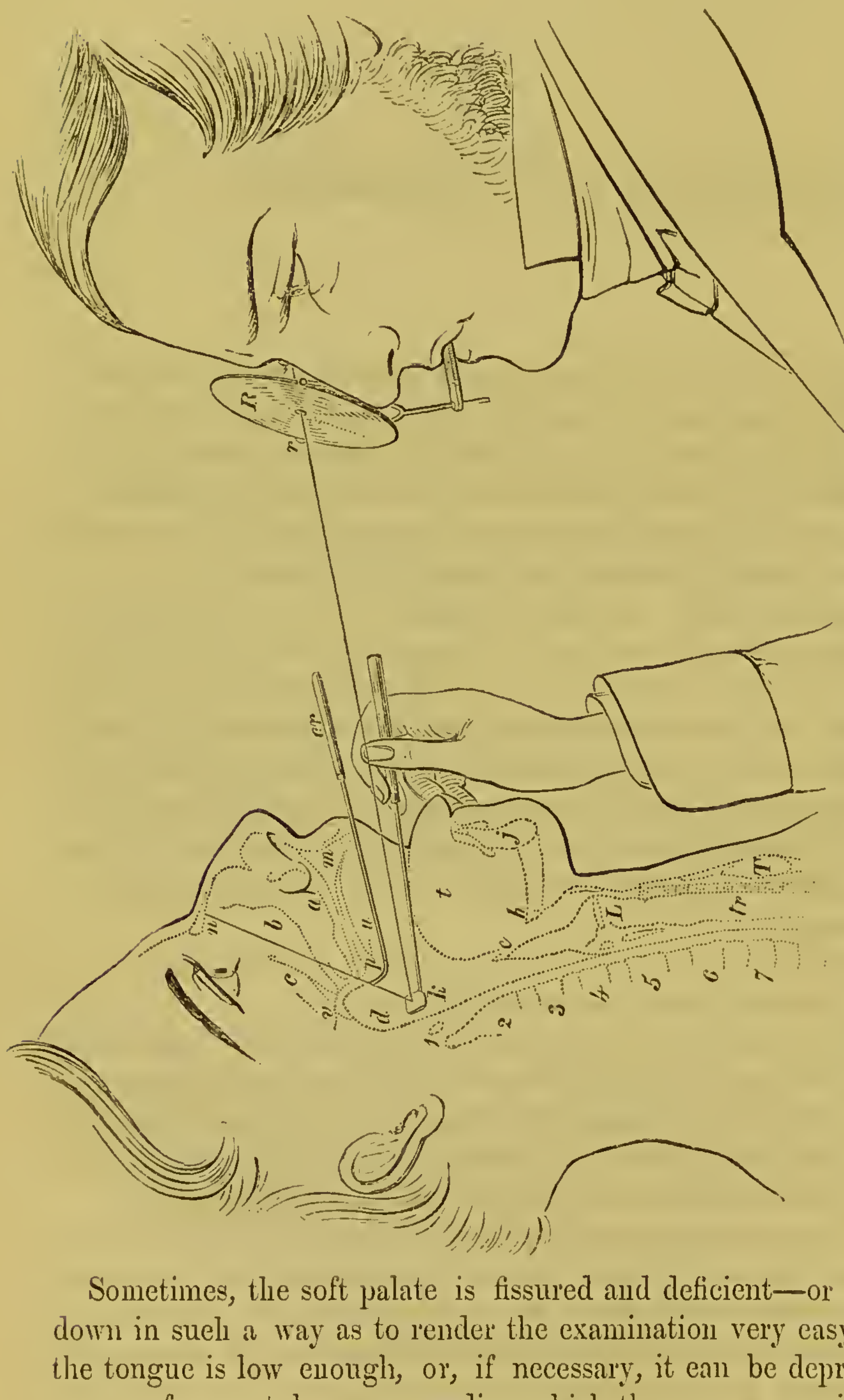

Fig. 30. 1 to 7, the bodies of the seven cervical vertebræ; *L*, *e*, the larynx and the epiglottis with a section of the cartilages; *tr*, the trachea; *T*, the thyroid body; *h*, a section of the hyoid bone; *t*, the tongue; *j*, the inferior maxillary bone; *m*, the palate process of the superior maxillary bone; *n*, the nasal bone of the left side; *v*, posterior or pharyngeal edge of the vomer; the external wall of the left nasal fossa is indicated by dotted lines; *a*, the inferior; *b*, the middle; *c*, the superior spongy or turbinated bone; the superior meatus is between *c* and *b*; the middle between *b* and *a*; the inferior between *a* and *m*; *p*, *u*, the soft palate and uvula; *d*, the opening of the left Eustachian tube; *cr*, the palate hook; *k*, the little rhinoscopic mirror; *R*, the large reflector perforated by a round hole in the centre; *r*, a little screw to fix the axis on which the reflector moves.

Sometimes, the soft palate is fissured and deficient—or it hangs down in such a way as to render the examination very easy; whilst the tongue is low enough, or, if necessary, it can be depressed by means of a spatula—a proceeding which the person examined may

readily manage himself. Under these circumstances the palate-hook is superfluous.

Should this not be the case—or, if a perfectly unrestrained view is desired—the soft palate must be *artificially* removed from the posterior wall of the pharynx.

This can be accomplished, as I have already pointed out, if the velum—*either voluntarily relaxed, or lowered by intonation of nasal sounds*—is gently lifted up and drawn forwards by means of a flat hook, the size and curvature of which must *vary* according to the proportions of the parts.

The difficulties of this and all other manipulations for the removal of the soft palate, are sometimes certainly very considerable—especially if the individual we have to deal with is irritable, awkward, resistant, or whose fauces are of unfavorable proportions. The *inexperienced* observer finds these difficulties almost insurmountable; but, with the *experienced*, this is not the case.

It is really incredible what the steady and easy hand of a careful and experienced observer can undertake with the organs under consideration, and what the latter learn to undergo in comparatively a short time. This observation cannot be too frequently repeated; and experience has shown its value in laryngoscopy.

At the commencement, rhinoscopic (as well as laryngoscopic) attempts which are undertaken on the living, are, as has been stated before, generally unsuccessful and discouraging. He who possesses neither discretion nor sufficient discernment to discover the chief reason of repeated failures that are due to his own awkwardness and inexperience, and who also does not possess sufficient energy and perseverance to acquire the necessary dexterity, will undoubtedly very soon lay aside his investigations, and, sooner than doubt his own ability, will declare rhinoscopy, as well as laryngoscopy, completely worthless and impracticable.

To set ourselves aright, we must commence by examining good drawings, and by experimenting on the dead; then only can we advance to the living—endeavouring to discover such individuals for our first experiments who are particularly suited for them by their possessing wide and insensible fauces.

A certain and harmless local anæsthetic for the fauces and soft palate would prove of great value in some difficult cases, even to the experienced, as rendering the examination easier—and would also materially aid the opening of the nasal valve (velum).

I must not omit mentioning that last spring, when in Paris, I was reminded of the "bromide of potassium, which had proved useful to Professor Gosselin, as a local anæsthetic, especially in the pharynx, during the operation of staphyloraphy." ('Gazette Médicale de Paris,' 14 Avril, 1860, p. 223); and that I had previously, in 1858, pointed to the composition of ether and chloroform, used by Weiger, a dentist of Vienna, as likely to be useful for this purpose.

Whatever the future may develope in this respect, we shall still find persons in whom the rhinoscopic and laryngoscopic methods will fail; but this want of success exists in all other modes of exploration, besides the present.

But when we have succeeded in removing all obstacles leading to the parts which we desire to inspect, and have regulated the light in a manner previously described, it becomes a comparatively easy matter to introduce the mirror, and to investigate the different parts of the pharyngo-nasal vault and nasal fossæ.

As a matter of course, all the general rules given for the use of the laryngoscope, apply equally to rhinoscopy. In the latter, as well as in the former, any person who has made himself familiar with the laws of catoptrics—of the topography of the respiratory organs—and who does not act without proper consideration, will very soon become aware of what he desires to perform.

It will now be expedient to direct the attention of the reader to the accompanying woodcut (Fig. 31).

The drawing shows a section of the head and neck, through the median line—the head thrown well backwards. All the parts of the external wall of the left nasal fossa, namely, the turbinated bones, the meatuses, and anterior nasal opening, are marked by dotted lines on the septum narium. The velum and the uvula are drawn out of their normal position (*pu'*) by a palate-hook (*k*), and turned forwards and upwards—the tongue being sufficiently depressed, the mouth, fauces, and nasal cavity, communicate freely.

If, under such circumstances, a little mirror is placed at *x*, and *yx* be the direction of the light and sight, then the observer must see in the direction *xn*, through the posterior nares into the nasal fossæ, which are visible throughout their *entire* extent. If the nasal fossæ are wide enough, the sight may extend along the septum as far as the nasal bones and the lamina cribrosa. The bottom of the nasal cavity, and the greatest part of the inferior meatus, remain in-

visible; but the *three* turbinated bones, and *both* of the superior meatuses are visible, to a more or less extent.

FIG. 31.

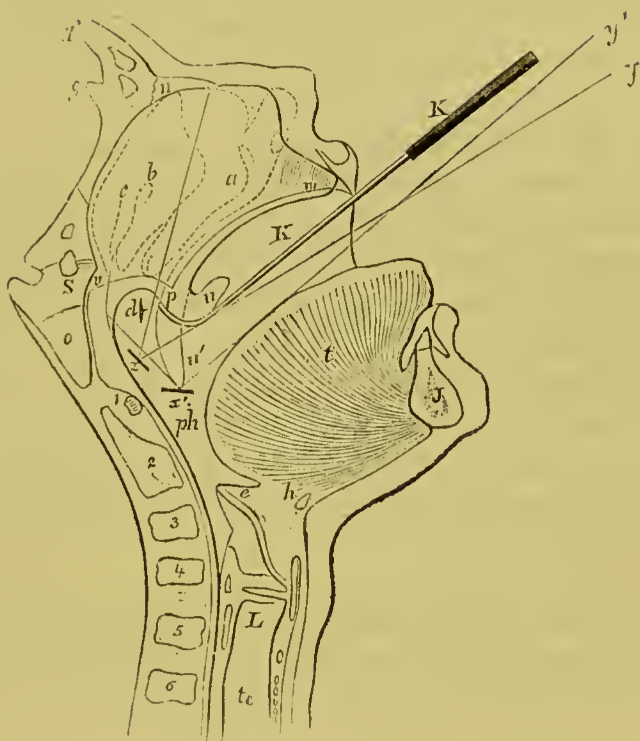

Fig. 31. 1—6, section of the six upper cervical vertebræ; *o*, a section of the basilar process of the occipital bone; *s*, a section of the body of the sphenoid bone and sinus; *g*, a section of the crista galli of the ethmoid bone; *f*, a section of the frontal bone and sinus; *n*, os nasi of the left side; *m*, the palate process of the superior maxillary bone separating the mouth from the nasal fossæ; *v*, the posterior or pharyngeal edge of the vomer; *d*, the opening of the Eustachian tube; *pu'*, a section of the soft palate and uvula, indicating the normal position of these parts; *pu*, the soft palate and uvula drawn forwards and upwards by the hook (*K*); *ph*, the pharynx; *t*, the tongue; *j*, a section of the inferior maxillary bone; *h*, a section of the hyoid bone; *eL*, a section of the epiglottis and larynx; *tr*, the trachea. The external wall of the left nasal fossa is indicated by dotted lines; *a*, *b*, *c*, the turbinated bones; *x*, *x'*, two different positions of the rhinoscopic mirror; *xy*, *x'y'*, two different directions of the light and sight.

If the mirror is inclined more to one side, it brings into view the lateral wall of the pharyngo-nasal vault, and the orifices of the Eustachian tubes.

If the mirror is placed in a lower and more horizontal position, as at *x'*, and the direction of the light and sight, equal *y'x'*, the posterior and upper wall of the pharynx is then illuminated and reflected; under these circumstances, the posterior convex surface of

the velum covers the lower part of the posterior nares, and limits considerably the view into the nasal cavity.

If the mirror is now brought by degrees from a lower into a higher position (from x' to x), and at the same time its inclination and the direction of vision and light be likewise changed in all possible ways, it is evident that the whole of the space that is at all accessible to the eye, must present itself from the most varied points of view.

The space that is susceptible to vision in one and the same individual, will, of course, depend upon the opening of the mouth, the isthmus faucium, and the nasal valve (velum); also upon the position of the nasal fossæ, relative to the pharynx, which is shown, by the way in which the base of the skull is bent towards the axis of the vertebral column.

For the special examination of the different parts, I consider a more minute description to be useless and superfluous to a *thinking physician*, unless some never-failing directions could be given which, considering the numerous diversities presenting themselves, is a matter of impossibility.

Finally, I will bring before the reader a few rhinoscopic cases, which came under my observation, not that they are of great pathological interest, but because they prove the possibility of applying the rhinoscope to medical purposes.

CASE I.—*Slight deafness, rhinoscopy, good view of the walls of the pharyngo-nasal cavity, &c.*

At the hospital of the Val de Grace in Paris, where (April 18th, 1860,) I demonstrated my different methods of application of the Liston-Garcia's principle in presence of Dr. Michel Levy and many other military physicians, a slightly deaf person was brought before me for the inspection of his pharyngo-nasal vault.

I readily succeeded without difficulty in making the first attempt; the patient depressed his tongue himself with a rectangularly bent spatula; I then took the palate-hook with my left hand, and introduced with the right, one of my small laryngeal mirrors.

The mucous membrane which covered the walls of the pharyngo-nasal cavity, the posterior nares, &c., was everywhere observed to be of a lively-red hue, associated with some slight swelling. The orifices

of the Eustachian tubes were visible and entirely free, but their margins were thickened.

Several persons standing near me participated in this view, which was considered highly satisfactory through the clearness and ease with which it was obtained, although the diagnosis was rather of a negative character.

Case 2.—*Large pharyngo-nasal polypus, seen by the aid of the rhinoscope.*

On the 10th August, 1860, I examined a man at the Charité Hospital in Paris, in presence of a large auditory, who composed the clinic of M. Velpeau. Some time previous the presence of a great polypus of the pharyngo-nasal cavity had been made out.

I succeeded with the greatest facility in the application of my method, as in most of the cases of laryngo-nasal polypi, and by the aid of a brilliant sunlight was enabled to show the lower part of the polypus in its natural condition and colour very distinctly; but its large size made it impossible to see the upper part and the point of its attachment.

The undoubted utility of the rhinoscope in cases where the tumours attain such a large size, consists in its permitting of thoroughly examining the visible surface, and of applying a sound or the galvanic cautery, *assisted by the eye.*

I regret very much that this case was not submitted to operation during my stay in Paris, as a later examination would have enabled me to give an exact description of its result, the point of attachment of the polypus, the condition of its remains, &c., &c. This refers to all similar cases, as an illustration of the utility of the method after the operation has been performed.

Case 3.—*Tumour occupying nearly the whole of the pharyngo-nasal cavity.*

On September 18th, 1860, I examined Dr. P. at Pesth, who felt an obstacle in the nose during breathing, and whose voice possessed an obvious nasal twang.

The inspection of the pharyngo-nasal cavity could be made without difficulty, and I could observe the presence of a greatly developed growth, which nearly filled up the whole pharyngo-nasal cavity.

On examining it more closely, and *sounding it by the aid of the*

mirror, it was felt to be a whitish, smooth tumour, filled with some kind of fluid.

I was prevented from further examination of this cavity, leaving Pesth soon after, but I recommended this gentleman, who is a physician, to make himself acquainted with the use of my apparatus for autolaryngoscopy (see Chapter III, Sections III and V), and to try and examine this curious growth more closely, by autoscopy, and to watch any changes that might occur.

Case 4.—*Frequent catarrh of the nose and fauces; slight deafness; a tumour dividing the pharyngo-nasal cavity into two parts.*

The patient in this case I examined in Prague, in December last. He was a young man who has frequently suffered from severe catarrh of the fauces and nasal fossæ, and he complained of his hearing being weak, and an unpleasant sensation at the upper part of the pharynx.

A rhinoscopic inspection showed the mucous membrane to be very red and thickened, covered by a copious secretion of mucus, and with enlargement of the follicles. Up to the level of the orifices of the Eustachian tubes, and concealing them from view, was a semilunar projecting swelling of the mucous membrane, which divided the pharyngo-nasal cavity posteriorly, as it were, into an upper and a lower part.

I have not had the opportunity of examining this patient since that time.

J. N. C.

Prague; *January*, 1861.

ON

THROMBOSIS

OF THE

CEREBRAL SINUSES.

BY

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MDCCCLXI.

ON

THROMBOSIS OF THE CEREBRAL SINUSES.

A case of formation of a thrombus in the superior longitudinal sinus, which came under observation amongst our out-patients in the course of the present year, together with some cases of a similar kind communicated by Dr. Gerhard, in the 'Deutsche Klinik,' some time ago, led to the publication of the following paper.

George Schäfer, aged nine months, came under clinical treatment January 13th, 1858. The child was in good condition and at the breast. Since the 6th ult. the parents had observed a furuncular inflammation in the upper third of the anterior part of the right thigh. On examination, extensive induration and swelling of the subcutaneous cellular tissue in the anterior and outer part of the thigh, reaching as far as the knee, was observed. The skin of the part was of a dark-red colour, and at some points an indistinct fluctuation was already perceptible. The child was very restless and feverish, and had great thirst, but continued to take the breast, from which a portion of its nourishment was derived, well. Poultices were ordered to be applied. On the 15th, transient general convulsions supervened, with constant feverishness. The inflammatory induration had now for the most part disappeared, and there was evident fluctuation. The abscess was, therefore, opened above the knee, and a counter-opening made in the neighbourhood of the hip-joint. About half a pint of good, creamy pus, with shreds of dead cellular tissue, was discharged. The wounds were kept open with lint, and a light compressory bandage put on. The poultices were continued. On the 16th, a large quantity of pus came away; the child was very restless, and on this day the first incisor tooth of

the lower jaw was cut. On the 17th, its condition was much the same. On the 18th, the discharge was more scanty, thinner, and of a bad colour. The edges of the wounds had a livid appearance. In the course of the day a slight cough came on. On the 19th, there was great collapse, with dulness of the eyes, and the act of sucking was, for the first time, very imperfectly performed. There was great restlessness and constant moaning, with persistence of the cough. Ordered decoction of cinchona, with Malaga wine. Towards evening transient epistaxis supervened, and, according to the parents' account, the urine was tinged with blood. Death occurred soon after 9 p.m. quietly, without convulsions having preceded it.

Post-mortem appearances thirty-six hours after death.—The body was tolerably fleshy. No rigor mortis. *Head.*—The greater fontanelle is open to the extent of half an inch, and somewhat depressed. Bones of the skull normal. In the anterior portion of the superior longitudinal sinus is found a firm, three-cornered, pale clot of blood, completely filling the sinus and adhering to its walls. This was found, on a closer examination, to be laminated, and contained in its interior, where it was somewhat softer, thickish, dark-coloured cruor. In the posterior portion of the sinus the thrombus does not occupy its whole calibre; it is also softer there, and consists of buffy fibrin infiltrated with serum. In the left lateral sinus is also found a fresh, soft, dark-red clot, while the right lateral sinus is filled with dark, fluid blood. The veins terminating in the superior longitudinal sinus contain tough, firm, colourless clots. Venous hyperæmia and slight œdema of the pia mater. The brain is strikingly firm for the age of the child. The ventricles are small, and contain only a few drops of serum.

Chest.—In the lower lobe of the left lung are found several small patches of lobular pneumonia, surrounded by lung-tissue in a state of atelectasis; in the upper lobe compensatory emphysema. The middle lobe of the right lung is in a state of complete atelectasis. The bronchial glands around the left bronchus are in a state of recent swelling.

Heart.—Contains no blood or clots. The mitral, tricuspid, and aortic semilunar valves show gelatinous thickening. On the first there are seen whitish, firmly adherent excrescences, partly smooth, partly ragged.

Abdomen.—Slight fatty degeneration of the liver. The spleen

very large, soft, and contains much blood. The mucous membrane of the stomach and intestinal canal presents nothing abnormal, with the exception of a hyperæmic distension of one of Peyer's patches just above the ileo-cæcal valves. The mesenteric glands are somewhat enlarged. Kidneys normal.

In the upper part of the right thigh there is extensive disorganization of the cellular tissue beneath the skin and between the muscles; in some places the fascia lata is seen to be perforated by the pus. No thrombus could be discovered in the venous trunks of this extremity.

Having given this case, I shall venture to add some remarks on thrombosis generally, and especially on thrombosis of the sinuses of the brain.

A slight glance at the history of the formation of coagula in the veins shows that it is closely connected with that of phlebitis, and various views have prevailed in reference to their origin. The clots having at first (Hunter) been regarded as an exudation upon the inner surface of inflamed veins, it was afterwards ascertained that they are true coagulations of blood, and the notion that coagulation of the blood in the veins is the immediate consequence of phlebitis, which was supported by the authority of Cruveilhier, prevailed generally. It was reserved for Virchow, in his classical work on thrombosis, to clear up the matter and to show that, in a large number of cases, the coagulation of the blood in the veins precedes the inflammation in them, while primary phlebitis, with subsequent coagulation of the blood, much more rarely occurs.

The post-mortem appearances given above present a case of the first kind. The proof lies in the complete absence of any appearance of inflammation in the walls of the sinus, where filled by the clot, or in its neighbourhood. That the clot was not formed after death is shown by its laminated structure, its firmness and dryness, its colour, its richness in fibrin, and the circumstance that it completely filled the sinus and was adherent to its walls.

The innate and immediate cause of the coagulation of that portion of the blood which we call fibrin is, indeed, still entirely unknown, but we know, from experience, that the coagulation is caused chiefly by two circumstances, viz.—(1) by the retardation and interruption of the motion of the blood in the vessels, and (2) by the contact with

the constituents of the atmosphere.[1] To the circumstances just mentioned must be added a property of the blood important for the growth of thrombi, the rudiments of which are mostly small, viz., the property of a tendency to coagulation from coming into contact with clots already formed, or with rough surfaces and foreign bodies which find their way into the vessels, as obtains in the process of crystallization. Lastly, it may fairly be assumed in many cases that an increased coagulability of the blood during life, as observed in various specimens of blood taken from the vessels, favours the formation of clots within the vessels, although we must be very cautious before we make such an assumption in a given case.

Virchow has already called attention to the circumstance that there are three localities in the body which, from their anatomical character and position, especially favour a retardation of the motion of the blood, and consequently its coagulation during life, viz., the veins of the lower extremities, the veins of the true pelvis, and the sinuses of the brain, and of the latter more particularly the longitudinal and lateral sinuses. In the sinuses it is the dilatations of calibre and projecting partitions in the walls which are said to favour coagulation. To this I would add that the peculiar form of the canals in this portion of the venous system and the relations of circulation proper to the cranium furnish further elements which may tend to retard the current, and thereby favour coagulations in the sinuses. The form of the canals, which in other portions of the venous system is for the most part circular, is in most of the sinuses quite another, and the thoroughly three-cornered form of the canal of the superior longitudinal sinus deviates, above all, from the ordinary conditions. By this deviation from the circular form of the canal the relation of the walls of the sinus to the current of blood flowing through it is essentially changed, and that in such a manner that the proportion of surface of the walls of the sinus to the quantity of blood is greatly increased. But it is clear that in vessels whose form differs from the cylindrical, the frictional resistance and

[1] Brücke has, it is true, recently shown, by his skilfully conducted investigations, that the death of the walls of the vessels is closely connected with the coagulation of the blood within those vessels; but the results of his inquiries apply chiefly to the phenomenon of coagulation in the dead subject, and not to the coagulation of the blood in living vessels.

adhesion to the walls must increase, while the rapidity of the current, *cæteris paribus*, must decrease. A further circumstance which must tend to retard the current of the blood is the peculiar manner in which the venous trunks discharge their contents into the superior longitudinal sinus, the stream in them being directed mostly at a right angle, and partly even at an obtuse angle to the current in the sinus. It follows from this that even under normal conditions the rapidity of the current in the sinus, especially in the superior longitudinal sinus, is comparatively slight. If pathological conditions supervene, which tend to diminish the force of the general circulation, the retardation in the sinus will become more considerable, and a state of things very favorable to coagulation be produced.

Diseases which cause a diminution of the mass of the blood generally, as *e. g.* losses of blood, profuse diarrhœas, &c., must have a similar effect. This depends upon the circumstance that the sinuses, from the great tenseness of their walls, are capable of very little dilatation, and may almost be regarded as rigid tubes, upon which the absence of atmospheric pressure within the cranium cannot have even an indirect effect, and which, from the complete want of muscular elements, are incapable of any active contraction. But if, when the supply of blood to the brain is diminished—as must necessarily occur on a diminution of the general mass of the blood—their volume cannot be lessened, a retardation of the current becomes inevitable. Lastly, an increased viscidity of the blood, such as occurs in certain diseased conditions, will suffice to retard its current in the blood-vessels, and therewith in the sinuses of the brain, and favour the formation of thrombi. It appears, however, as if a very high degree of thickening of the blood, such as is met with, for instance, in the cold stage of Asiatic cholera, were unfavorable to firm coagulation of the blood. At least, the tarry, imperfect coagulation of the blood, and the infrequency of firm, buffy clots in most of the bodies of persons dying during the cold stage, with the very incomplete, chiefly gelatinous, coagulation of blood taken from the veins, speak in favour of the above view, although it cannot be denied that chemical and still unknown modifications of the blood in this disease may also favour this result.

It frequently happens that several of these circumstances coincide and support each other's influence, and among these I would specially allude to profuse fluid discharges, which cause simultaneously a

diminution of the mass of the blood, a thickening of the same, and a decrease in the power of the heart.

With these preliminary remarks I address myself to the facts of the matter as we find them mentioned in works on the subject.

From the works[1] to which I had access I have collected fifty-seven cases in which the formation of thrombi in the sinuses of the brain is mentioned. A comparison of these cases in reference to their causation shows that, in thirty-two, the thrombosis was the result of gangrenous, erysipelatous, and suppurative inflammations of those parts of the body (neck, face, orbits, bones of the skull, brain and its membranes) whose vessels are in close connexion with the sinuses; in four it appeared to result from a diminution of the calibre of the sinus, by the intrusion of foreign bodies and tumours, or from pressure from without upon the sinus or upon the internal jugular vein. In fifteen it appears to have been occasioned by circumstances which lessened the force of the circulation, namely, by previous debilitating diseases in individuals for the most part feeble already (aged persons and children). Lastly, in six cases nothing positive as to the cause could be ascertained from the history.

In the following sections I shall give the cases in the various groups, partly in detail, partly in an abreviated form, and append the reflections arising from them concerning the nature of the causes, the anatomical position, and the changes in the thrombi within the sinuses, as well as concerning their consequences.

I. *Thrombosis of the Sinuses from Inflammatory Processes in the neighbourhood of them.*

Caries of the bones of the skull furnishes the largest contingent to this section, viz., twenty-seven cases out of thirty-two, and amongst these, again, caries of the petrous portion of the temporal bone, the

[1] Unfortunately I could not obtain Tonnelé's work in the 'Arch. gén. de Méd.,' vol. xix, ser. 1, 1829, p. 610, and in the 'Journ. hebdom.' of February 5th, 1829, the cases mentioned in which are only known to me from Lebert's paper ('Virch. Archiv,' 8, 381), and from the communications by Albers ('Rust's Mag.,' 41, 139), and from Rillict and Barthez, in their work on the 'Diseases of Children.'

consequence of so-called otitis interna, is most largely represented, viz., twenty times.

Lebert,[1] in a very valuable work, has laid great stress upon the circumstance that phlebitis of the lateral sinus is frequently the connecting link between otitis interna and the cerebral and pyæmic phenomena which so frequently follow it.

In each of the twenty cases mentioned there existed thrombosis in the lateral sinus of the side affected with caries. Only in one case observed by Stannius[2] was the cavernous sinus the seat of a suppurative phlebitis, which extended to the vena ophthalmica and vena facialis anterior and its branches. In this case it might certainly be doubtful whether the affection of the cavernous sinus had not been produced by erysipelas of the corresponding side of the face, from which the thrombosis and phlebitis had extended to the cavernous sinus by means of the anastomosis of the vena ophthalmica with the vena facialis anterior, had not the superficial caries of the petrous portion of the temporal bone (the exact point of which is not mentioned), which led to the destruction of the dura mater and to an abscess of the brain, presented itself as the more probable starting-point.

In many cases the thrombosis is confined to the lateral sinus, in others, and indeed in the majority, it extends further. Thrombus-formation is generally observed simultaneously in the internal jugular vein, and sometimes extends to the opening into the subclavian vein, and in one case even reached the vena cava superior near the spot where it communicates with the right side of the heart. In a direction upwards, *i. e.* in an opposite direction to the current of the blood, thrombi are most frequently found in the superior petrosal sinus, where, however, they are generally not to be regarded as having extended from the lateral sinus, but as being produced by the same cause as those observed in the latter. Rarely are thrombi found simultaneously in the lateral sinus of the other side, in the petrosal and cavernous sinus, and *most rarely in the superior longitudinal sinus*. In one case only is it mentioned that the thrombus had extended from the torcular Herophili into the extremity of the longitudinal sinus, and it appears rare to find the whole of the lateral sinus in a state of thrombosis as far as the torcular. As regards the nature of the thrombi, they were, in almost all the cases, in a state of

[1] 'Virchow's Archiv,' 9, 81.

[2] 'Krankhafte Verschliessung der grösseren Venenstämme,' 118.

suppuration, frequently also mixed with pseudo-membranous clots and fresh coagula of blood. The inner coat of the sinus is frequently reported to have been thickened, or covered with flat coagula. In four cases only had the thrombi not yet reached the suppurative stage; thus, in a case of Puchelt's,[1] in which only two valve-like clots were found, which almost entirely obstructed the sinus; further, in a case by Abercrombie,[2] and in one by Bruce,[3] in which the walls of the sinuses were mentioned as thickened and changed, and the canals filled with clots similar to those found in aneurisms. Lastly, Heusinger[4] relates a very interesting case, in which a firm thrombus completely filled the lateral sinus, and the dura mater was perforated at a point in the sinus corresponding to the posterior part of the petrous bone. To the extent of half an inch in length, the inner coat of the sinus was entirely destroyed at this point, and ragged granulations enveloped in pus, sprouting out towards the carious depression in the petrous bone, completely filled the canal. At the same point there was also a communication of the carious petrous bone with an abscess of the right cerebellum. This case is worthy of notice, in as much as the nature of the thrombus shows us that its production could not have depended upon the introduction of pus following rupture of the walls of the sinus, in which case hæmorrhage must necessarily have ensued. Besides this case, three others are met with in which mention is made of a perforation of the walls of the sinus, and of a communication with the carious portion of the petrous bone, admitting of the intravasation of pus into the sinus from without. But in the other cases the thrombus was already in a state of suppuration; thus, in Hooper's,[5] in which both lateral sinuses were filled with pus and covered by false membranes; further, in Bruce's,[6] in which the posterior part of the right petrous bone was carious and perforated at one point, and the walls of the sinus corroded; and lastly, in a case taken from the 'London Medical and Surgical Journal,'[7] in

[1] 'Venensystem,' 2, 177.

[2] 'Die Krankheiten des Gehirns und Rückenmarks.' Deutsch von v. d. Busch, 49.

[3] 'London Med. Gaz.,' 1841. Lebert, loc. citat., 420.

[4] 'Virchow's Archiv,' xi, 92.

[5] 'Morbid Anatomy of the Brain,' Cruveilhier, vol. viii, pl. 4, p. 3. Remark.

[6] Lebert, loc. citat., 419.

[7] Vol. v, 679. Lebert, loc. citat., 421.

which very stinking, cheesy pus, mixed with blood, was found in the left lateral sinus, which communicated with the internal ear. In other cases the suppurative inflammatory process has approached very closely to the sinus, without, however, implicating it; the dura mater is sometimes found enveloped in pus in the groove provided for the reception of the sinus at the posterior part of the petrous bone, or is at least detached from the bone, and communications exist with the carious internal ear and cavity of the tympanum. The dura mater over the petrous bone often appears discoloured, of a grayish-green or blackish colour, and covered with purulent exudation, or it is thickened, and adherent to the arachnoid, and covered with spongy granulations. Lastly, in other cases, so close an encroachment of the inflammatory process upon the walls of the sinus does not occur, so that a direct communication between the caries and the thrombosis of the sinus, as in the cases mentioned above, does not exist.

The anatomical changes met with in these cases in the brain and its coverings are mostly purulent meningitis and abscesses in the brain, of which several are sometimes found simultaneously. The latter are frequently connected directly with the carious parts (as, for instance, in the above-mentioned case of Heusinger's), or are met with, at least, in their immediate vicinity, or, lastly, we observe them at a considerable distance without demonstrable direct connexion. Sometimes, however, the brain is only slightly discoloured or a little soft at the point of contact with the diseased bone, and in some places is even found quite normal. Œdema and hyperæmia of the pia mater are frequently met with, also serous and sero-purulent effusions into the lateral ventricles. The comparative infrequency of extravasations is striking; I nowhere find an effusion of blood in the brain mentioned, and only twice are slight extravasations in the pia mater reported (Lebert and Abercrombie).

As regards the changes in other organs which may be brought into connexion with the affections of the petrous bone and of the sinus, these are mostly of a secondary character, and consist especially in metastatic abscesses and hæmorrhagic infarction of the lung, often combined with suppurative or ichorous pleurisy and pericarditis, or in pleurisy alone. They occur in more than half the cases. The absence of metastatic deposits in other organs, especially the liver, appears striking, but it must be remarked that an exact report of the state of the other organs in the body is often wanting; on the

other hand, the liver and spleen are often mentioned as enlarged and hyperæmic, and the latter as softened simultaneously with enlargement of the glands of the small intestines.

As regards the symptoms during life I may be brief, and refer my readers to the excellent and comprehensive description in Lebert's work. Otorrhœa was never absent during life, and pain in the affected ear and corresponding side of the head were generally present; abscesses in the neighbourhood of the mastoid process are frequent occurrences, because the caries and purulent infiltration easily extend from the petrous bone to the spongy cells of the mastoid process. One symptom proper to the disease and obstruction of the sinus, and furnishing a certain diagnosis of them, was not observed in the cases above mentioned. Heusinger[1] only mentions considerable distension of the frontal vein during life, which might be regarded as a consequence of the thrombosis of the sinus.

That changes in the brain and its membranes so considerable as those mentioned above should give rise to cerebral symptoms may be assumed *a priori*, and thus we actually observe all the phenomena of excitement, such as delirium and convulsions, and of depression of the functions of the brain, such as paralysis of the motor and sensory nerves, stupor, sopor, and coma, although sometimes, with very considerable lesions of the brain (two abscesses in its substance), any special cerebral symptoms were wanting, as in Heusinger's case. As might be expected also with the frequent occurrence of metastatic and secondary inflammations, the pyæmic fever symptoms and the phenomena proper to the organs secondarily affected, such as cough, pain in the side, diarrhœa, &c., are mentioned in more than half the cases. But I must remark, as others have done already, that the post-mortem appearances do not always correspond to the phenomena observed during life, as it sometimes happens that, with decided pyæmic symptoms during life, no metastatic processes are met with in the dead body, and, conversely, still more frequently metastases were present in the dead body while none of the peculiar phenomena were observed during life.[2]

[1] Loc. citat.

[2] As a detailed communication of the twenty cases of this kind examined by me would be of no further use, I refer to the literary sources to which I had recourse. These are—1. Puchelt, 'Venensystem,' ii, 177. 2. Abercrombie, 'Krankheiten des Gehirns und Rückenmarks.' Deutsch von v. d. Busch, 49

To these twenty cases of thrombosis of the sinuses, with caries of the petrous bone, may be added three others, in which various causes had produced caries of the bones of the skull, with formation of clots and phlebitis of the sinuses.

Case 21.[1]—A woman was seized with pain in the right side of the head and in the ear, after taking cold during her confinement, which was soon followed by feverish symptoms, and finally by acute indications of pyæmia, which caused death nearly four months after delivery. At the autopsy, extensive caries of the temporal bone was found, which extended as far as its point of contact with the occipital bone, and destroyed the cartilages of the suture, with purulent phlebitis of the lateral sinus, and thrombosis of the jugular vein.

Case 22.[2]—A man, æt. 42, received an injury of the right parietal bone from a sabre cut. The bone was denuded of periosteum; an abscess formed around the wound; symptoms of bronchitis and of infiltration in the lower lobe of the right lung showed themselves at the same time, and death ensued in four weeks, with delirium and consequent coma.

Sectio.—The bone was found denuded to a large extent, and carious; the temporal muscles permeated by veins the seat of purulent infiltration, with the like infiltration of the diploë along the coronal suture in the frontal, parietal, and temporal bones. On the inner surface of the cranium were numerous sieve-like points of suppuration. Most of the grooves for the vessels on the inner surface appeared corroded, especially along the longitudinal sinus. The

and 58. 3. Heusinger, Virchow's 'Archiv,' xi, 92. 4. Lebert, ibid., ix, 413 and 415. 5. Bruce, 'London Med. Gaz.,' 1841, and in Lebert, loc. citat., 417, 418, 419, and 420. 6. Bright, 'Med. Reports,' ii, 66, and Lebert, loc. citat., 420. 7. 'London Med. and Surg. Journ.,' v, 679, and Lebert, loc. citat., 421. 8. Craigie, 'Pract. of Phys.;' Lebert, loc. citat., 422. 9. Sédillot, 'De l'Infection Purulente,' 320; Lebert, 426. 10. Lunier, 'Bulletin de la Soc. Anat.,' xxi, 177; Lebert, 430. 11. Le Maistre, 'Bull. de la Soc. Anat.,' xxiii, 18; Lebert, 430. 12. Smith, 'Dublin Journ.,' 1841, xix, 458. 13. Stannius, 'Krankhafte Verschliess. d. gr. Venenstämme,' 118. 14. Hooper, 'Morbid Anat. of the Brain,' 1826, Cruveilhier, vol. viii, pl. 4, p. 3. 15. Bednar, 'Die Krankheiten der Neugebornen und Säuglinge,' ii, 100 and 180.

[1] Puchelt, 'Venensystem,' ii, 178.

[2] Leubuscher, 'Klinik der Gehirnkrankheiten,' p. 235.

dura mater was infiltrated with pus. The veins, which were in many places distended, contained thick, yellow pus, which extended into the longitudinal sinus. The coats of these veins were thickened, the Pacchionian glands in the neighbourhood of the falx quite full of pus. The walls of the longitudinal sinus were for the most part healthy; at some spots, however, the mouths of the veins were thickened and mostly rough from exfoliation; at all these points, firm, yellowish-white, fibrinous clots were attached, partly flat, partly in the form of tufts hanging free in the blood of the sinus coagulated after death. These clots consisted of fibrinous capsules filled with fluid pus. The further channel of the longitudinal sinus and the lateral sinuses were full of recently coagulated blood. There was offensive purulent exudation in the sac of the arachnoid on the right side, and purulent infiltration of the membranes of the brain, the right hemisphere being strongly compressed by the exudation. The membranes of the left side looked dull; there were effusion into the ventricles, and metastatic abscesses of the lung and liver.

Case 23.[1]—A soldier, æt. 22, who had received a gun-shot wound of the right parietal bone, was soon afterwards seized with giddiness and accessions of fever. An abscess formed in the hairy scalp, and the wounded man died with symptoms of pyæmia, to which were added, on the 29th day after the injury, oppression of the chest and diarrhœa.

Sectio.—In the posterior part of the longitudinal sinus pure pus was found; in the anterior portion a firm clot, mingled with fleshy fibres. The bone at the point of injury was rough, and the dura mater at that spot discoloured. There were purulent meningitis with effusion into the ventricles, purulent pleurisy of both sides, metastatic abscesses in the lungs, and inflammation of the liver.

Injuries of the skull, followed by inflammation in the vicinity of them, even if caries of the bones of the skull, as in the two last-mentioned cases, does not occur, also sometimes lead to thrombosis of the sinuses, as will be shown in the following cases.

Case 24.[2]—A soldier, æt. 21, was wounded by a grenade in the right parietal bone. Swelling of the left parotid and of the neck followed, accompanied by accessions of fever and diarrhœa. The

[1] Schmucker, 'Chirurgische Wahrnehmungen,' 465.
[2] Schmucker, loc. citat., p. 126.

swelling of the neck subsided, and death took place on the 19th day.

Sectio. The cranium and dura mater were uninjured. In the superior longitudinal sinus were found pus and polypous clots, extending as far as the lateral sinuses. Purulent meningitis. Brain in the neighbourhood of the left fissure of Sylvius converted into a dark-brown mass. Beneath the situation of the external injury, a cavity was found in the right hemisphere of the cerebrum, which contained about half an ounce of congealed blood; round about it the brain was in a state of yellow softening. In this case the cause of the thrombosis of the sinus is not quite clear. It may have been occasioned by the inflammation of the external wound and consequent meningitis, or it may have had its origin in the hæmorrhagic deposit resulting from the violence, or lastly, been caused by the evident destruction of a portion of the brain in the neighbourhood of the left fissure of Sylvius by *contre-coup*. The extensive lesions of the brain found in this case as the immediate consequence of external violence, yet without injury of the bone, are worthy of remark.

Case 25.[1]—A man, æt. 54, suffered an injury of the occiput from the fall of a stone, which made a wound which suppurated slightly. Eight days later, rigors set in, accompanied by headache and other cerebral symptoms. Death occurred at the end of the third week, preceded by symptoms of pyæmia and coma. In the longitudinal sinus and in both lateral sinuses were clots in a state of suppuration, while the walls of the sinuses were thickened. At the base of the skull there was a fracture, the exact situation of which is not mentioned. Considerable hyperæmia of the brain. Also a large abscess beneath the pectoralis major muscle, and numerous abscesses in the liver.

In this case, also, the thrombosis may either have proceeded from the point of fracture, or have been intimately connected with the suppurating wound at the back of the head.

Case 26.[2]—In a man who had a cicatrizing scalp wound, rigors suddenly supervened; the edges of the wound assumed an erysipelatous character, the periosteum was seen to be detached from the bone,

[1] Lebert, loc. citat.

[2] Bérard, 'Bulletin de la Société Anat.,' x, 6; Lebert, loc. citat., 432.

and eight days after the appearance of symptoms death ensued, not preceded by any striking cerebral phenomena.

Sectio.—A membranous exudation was found upon the outer and inner surface of the dura mater, connecting it with the arachnoid. The superior longitudinal sinus was filled with pus. There were circumscribed purulent peritonitis and pleurisy, and numerous abscesses in the liver.

Although in this somewhat imperfectly reported case no mention is made of caries, the same appears very probable on account of the detachment of the periosteum; but, no doubt, the thrombosis must be referred to the inflammatory process in the wound.

Lastly, there exists another series of cases in which, without demonstrable caries of the bones of the skull, and without traumatic elements, through inflammation of external tissues which belong partly at least to the region of vessels of the sinuses of the brain, thrombosis of the latter was produced.

CASE 27.[1]—A scrofulous girl, two years old, who had a sore on the right occiput, died in an adynamic condition after the supervention of convulsions and cerebral symptoms.

Sectio.—The lateral sinus was distended to double its normal size, and full of stinking pus, which contained small solid clots with coagulated blood; the inner surface was lined with a false membrane. There was, moreover, purulent meningitis.

Lebert, who also gives this case, supposes the existence of caries overlooked by Tonnelé; but, although this is not improbable, and although we cannot regard the inflammation of the sinus in this case as a spontaneous one, as Tonnelé has done, it is not necessary to assume the existence of caries to explain the connexion, since the sinuses in part receive the blood from the integument of the skull (emissaria Santorini), by which means an intimate vascular connexion between the suppurating sore and the sinus is established, through which a thrombus might extend from without into the sinus.

CASE 28.[2]—Another girl, two years old, had freely suppurating

[1] Tonnelé, 'Archives Gén. de Méd.,' xix, sér. 1, 1829, 610; Lebert, loc. citat., 86.

[2] Tonnelé, in 'Rust's Magazine,' xli, 139, 1834 (nach Albers).

eczema of the hairy scalp. The discharge suddenly decreased, cerebral symptoms showed themselves which indicated an affection of the left hemisphere, and terminated in death.

Sectio.—In the posterior part of the longitudinal sinus was found a plug broken down by suppuration, the walls of the sinus were thickened, and the veins communicating with it were seen to be filled with stiff thrombi. Beneath the arachnoid on the left side there was an extravasation of blood.

As regards the origin of the thrombus, this case manifestly resembles the preceding one, and admits of precisely similar remarks.

Case 29.[1]—In a man, æt. 27, who had been exposed to severe cold, marked feverish symptoms were followed by headache, pain in the eyes, protrusion of the eyeballs, œdema of the eyelids, and a very painful swelling of the right side of the neck; indications of the existence of pyæmia supervened, and death occurred on the 23d day of the disease.

Sectio.—There was purulent phlebitis of the right cavernous sinus, of the veins opening into it, of the ophthalmic vein, the circular sinus, the petrosal and lateral sinuses on both sides, the jugular veins, the vena innominata dextra, and of the veins of the nape of the neck and the vertebral canal, besides abscesses in the cellular tissue of the orbits. There was also found circumscribed purulent meningitis, with superficial softening of the substance of the base of the brain on the right side, and a clot of blood upon the medulla oblongata. Further, metastatic abscesses and hæmorrhagic infarcta in the lungs, acute enlargement of the spleen, and distension of the kidneys.

Case 30.[2]—A man, æt. 26, who had suppurating parotitis penetrating into the meatus auditorius externus, consequent upon double pneumonia, died with indications of pyæmia and cerebral disease.

Sectio.—There were extensive disorganization and purulent infiltration of the tissues around the right parotid gland, the temples, the neck, and lower jaw; the articulation of the lower jaw and the external meatus were laid open by the pus. Broken-down thrombus

[1] Castelnau et Ducrost, 'Recherches sur les Abcès multiples,' Paris, 1846, 238; Lebert, loc. citat., 388.

[2] Virchow, 'Gesammelte Abhandlungen zur wissensch. Med.,' 620.

with thickening of the walls of the left jugular vein; a puriform plug in one of the lateral branches of the right jugular vein. In the longitudinal sinus, buffy clot, plugging up the openings of some of the arachnoid veins. These obstructions commence, as is seen on removing the dura mater, at the Pacchionian glands, which are surrounded by pus extending from the longitudinal fissure to the posterior border of the left hemisphere of the cerebrum; the right lateral sinus filled with fresh, gelatinous, buffy clot; in the *left*, on the contrary, as far as the jugular vein, besides fresh clot, older masses passing into a state of suppuration. Also in the veins of the dura mater in the right middle fossa of the base of the skull, and in both cavernous sinuses, sanious purulent masses. The right petrous bone discoloured, also the inner surface of the right wing of the sphenoid, and partly infiltrated with ichor. The Gasserian ganglion permeated by exudation and the second branch of the fifth nerve soaked in pus, which extends through the foramina outwards. Extensive exudative meningitis and at one point of the middle lobe of the brain, commencing encephalitis. Sanious lobular pneumonia; thrombosis of the veins of the right vesicula seminalis; abscess of the liver, with softening thrombosis of the hepatic vein. Enlargement of the spleen, and parenchymatous nephritis.

In the two last-mentioned cases, the extension of the thrombosis with puriform disorganization from external vessels to the cerebral sinuses is clearly demonstrable. In that of Castelnau and Ducrost, the starting-point is the inflammation of the cellular tissue of the orbit (as already correctly remarked by Lebert); but not the inflammation in the neck, as the first symptoms evidently showed themselves in the orbit, and the products of the inflammation were most advanced there. The thrombosis had extended from the ophthalmic vein to the right cavernous sinus and circular, petrosal, and lateral sinuses on both sides, and finally through the jugular vein as far as the vena innominata.

In Virchow's case, the origin of the thrombosis was the purulent parotitis and the sanious inflammation in its vicinity; the thrombosis had advanced into the cavernous sinus of the right, and afterwards of the other side, through the venous anastomoses in the inferior orbital fissure, and had extended to the lateral sinus and left jugular vein. We, therefore, see in this case the thrombosis extending in a striking manner, especially upon the side opposite to its starting-point. The

purulent plugs in the arachnoid veins of the left side, which extend as far as the longitudinal sinus, appear to have a special origin, probably in the extensive meningitis.

Cases 31 and 32.[1]—Hasse observed two cases, in one of which the phlebitis of the sinus resulted from phlebitis of the jugular vein; in the other, from a purulent exudation upon the arachnoid.

If we cast a retrospective glance at the whole of the facts given above, we may assume positively for all the cases—with some exceptions, already alluded to in the proper place, in which a different explanation is admissible—that the thrombosis and phlebitis of the sinuses stand in a causal connection with the inflammations existing within their radius of vessels, and both the course of events during life and the post-mortem appearances favour the view that the affections of the sinuses occurred later, in point of time, than the inflammations in the parts around them.

We might, therefore, easily be disposed to regard the thrombosis in the sinuses as a consequence of inflammation of their walls, *i. e.* as a secondary one. Independently, however, of the circumstance that the outer surface of the walls of the sinuses is, in very many cases, found unchanged, and does not stand in any direct contact with inflamed tissues, I believe, for further reasons, that another explanation is possible, which I consider the more admissible because the theory that the coagulation of the blood is a consequence of inflammation of the coats of the vessels is founded upon very unreliable data.

We are compelled to explain it by altered molecular attraction between the coats of the vessels and the blood flowing through them, or, in a word, by a contact influence (catalytic action); a kind of explanation which is rather suspicious, and which we should only be justified in adopting if hard pressed. I need scarcely remind my readers that an exudation of coagulated inflammatory products upon the inner surface of the walls of the vessels does not take place; that neither the purely fluid nor the cellular products of inflammation cause coagulation of the blood; and we see in the case communicated by Heusinger,[2] a fresh proof how, when an abscess pierces the walls of a vessel, a ready formed thrombus may have long been present.

[1] 'Patholog. Anat.,' i, 39.
[2] Loc. citat.

Very considerable changes of the inner coat of the vessels, even—such as we see in far-advanced atheromatous ulceration and calcification in the arteries—by no means certainly cause coagulation of the blood within them; indeed, it may safely be assumed that all the individuals in whom the formation of clots occurs under these circumstances live for a considerable time with such diseased arteries before coagulation of the blood takes place. Coagulation within the vessels, as a consequence of changes in their walls, is therefore far from occurring in every case, and when it happens it is frequently not until the changes have long existed.

It might further be added, perhaps, that the influence of the dead coat of a vessel upon the blood, demonstrated by Brücke,[1] would prevail also in the case of inflammation and disease of the venous coats. But if something of this kind may obtain, we must not forget that there is a great difference between the inflamed and the dead coat of a vessel, and that in Brücke's experiments the blood in the vessels and heart was at perfect rest, while we have to do with coagulation of the blood still in motion.

If, therefore, we have the choice of another mode of explanation, based upon clearly recognised causes of coagulation, we cannot well do otherwise than give it the preference.

As regards the formation of thrombi in the sinuses of the dura mater, sometimes met with after injuries of the head, Virchow[2] has already pointed out that a primary thrombosis of the veins of the diploë probably occurs in many cases from contact of the blood with atmospheric air, and simultaneous interruption of continuity in the vessels (hæmorrhagic thrombosis). From the peculiar nature of the veins of the diploë, and their close attachment to the unyielding bones, collapse of their walls is impossible. In cases of injury, therefore, gaping venous apertures present themselves, into which a hæmorrhagic thrombosis, formed externally to them, must readily extend and, by further propagation, reach the sinus. In thrombosis of sinuses, the result of the caries of the bones of the skull, these peculiar conditions of the veins of the diploë appear to me also to play an important part. The necrosis of the individual layers of bone, resulting from the process of ulceration in the bone, must necessarily diminish the supply of blood from the bone to the larger

[1] Loc. citat.
[2] 'Ges. Abh. zur w. Med.,' 619.

venous trunks, and it will even happen in many cases that the supply of blood from the bone to certain of these is completely arrested. But as a narrowing of the calibre of these vessels is impossible, as already remarked, a diminished or arrested supply leads to stagnation, and the formation of thrombus in them, which may extend into the sinus. We should thus have to regard the thrombosis in the sinuses in such cases as propagated, that in the small veins of the diploë as primary and dependent upon the comparative excess in their calibre.

A similar view may be taken of extensive sanious inflammations of the cellular tissue, inflammations which, as is well known, easily lead to phlebitis of the larger venous trunks and to the phenomena of pyæmia and metastatic deposits. In such phlegmonous inflammations a greater or less extent of venous radicles is for the most part soon destroyed by the inflammatory process, which rapidly spreads and leads to necrosis of the tissues, whereby the supply of blood to the small venous trunks is diminished or completely arrested, and stagnation of the blood in them takes place from the absence of the *vis a tergo*. It may, indeed, more readily occur here than in the veins of the diploë that these small vessels collapse and propel their contents into the next branch, by which means the formation of a thrombus in them is prevented. This may also be the reason why phlebitis does not always ensue in such cases. But if the process of inflammation has previously produced solid infiltration and thickening of the tissues around the small veins, it will happen here, as in the veins of the diploë, that, the supply of blood being diminished, the unyielding nature of the vessels will furnish conditions favorable to the formation of thrombus. These small thrombi grow, and finally reach the larger trunks, and phlebitis ensues in the latter. The reason why this consequent phlebitis mostly leads to suppuration and destruction of the coats of the vein, and but seldom assumes the so-called adhesive form, lies in the deleterious nature of the thrombus, which, arising in a deposit of various products of decomposition, conveys these by imbibition into the larger vessels.

Such a view of the origin of thrombosis and phlebitis in the vicinity of inflammatory processes would diminsh still more the range of so-called secondary thrombosis from inflammation of the walls of the vessels, since the coagulation of the blood in the small veins must be regarded as the consequence of stagnation, while that in the larger veins may be represented as occasioned by propagation (produced thrombosis), and the inflammation of the coats of the veins

may be looked upon as secondary. The inflammation of the outer coat of the small vessels could only be assumed as a determining condition for the formation of thrombus, in so far as the firm infiltration of the cellular tissue prevents the collapse of the veins. This view, against which many objections may indeed be urged, is by no means new, and we find allusions to it in the very suggestive 'Rational Pathology,' of Henle,[1] who did not overlook the favorable conditions for the formation of thrombus, which exist in the non-contractile veins as well as in the sinuses of the dura mater, the veins of the bones, and in veins of which the coats are paralysed or thickened and indurated. In the same work it is also remarked that in cases of stagnation in the capillary system of vessels from diminished *vis a tergo,* the blood of the veins originating there may lose its motion and coagulate, and inflammations of veins apparently result from stagnation in the capillaries.

But it is far from my intention to apply this theory to all cases of so-called secondary thrombosis, and I appreciate the objections which may be made to it. Above all, it may be urged that thrombi have not been observed in the small veins, and that this explanation is therefore inadmissible. But if we bear in mind how difficult it is to trace small vessels, and especially veins, in decomposed tissues, since the small venous trunks in which the thrombosis has had its origin have, in most cases, undergone decomposition in common with their thrombi before death occurred, this objection will lose much of its force. There have even been cases of phlegmonous inflammation in which it was impossible to discover any thrombi, or inflammatory changes in the *larger* vessels, but in which the presence of metastatic deposits in the lungs rendered the assumption of the previous existence and decomposition of thrombi in the *smaller vessels* necessary.

II. *Thrombosis of the Sinuses in consequence of a diminution of calibre from intrusion of foreign bodies and from compression.*

The formation of thrombus from the causes given in the heading to this section, is *a priori* very probable; although the few cases to be met with in the literature of the subject for the most part indi-

[1] Loc. citat., ii, 516 and 517.

cate other sources. In the case of the formation of clots resulting from the intrusion of foreign bodies and tumours, besides the stagnation of the blood dependent upon the narrowing of the channel, we must take into consideration the contact of the blood with a foreign body as favouring coagulation.

CASE 33.[1]—A man, æt. 23, had had otorrhœa for twelve months. After some general uneasiness, repeated, tolerably well-marked rigors set in, followed by heat and sweating. Headache, nausea, and inflammatory swelling of the salivary glands supervened, while the feverish attacks became less regular. There was great restlessness, with pain in the neighbourhood of the liver and swelling of that organ. Well-marked jaundice, diarrhœa, collapse, and death at the end of the fourth week.

Sectio.—In the interior of the left petrous bone was found a large chole-steatoma, which had perforated the bone at two points on its posterior and anterior surface, and thus infringed directly upon the cranial cavity. The upper border of the petrous bone formed a mere bridge over the cavity in which the tumour lay. The brain at this point presented a grayish discoloration. At the anterior part of the bone the much thickened dura mater was perforated in a round hole, and the posterior part of the perforation corresponded to the bend of the lateral sinus, which was entirely obstructed posteriorly by old, partly colourless, breaking-down, adherent plugs, while anteriorly, towards the sinus jugularis, extended a blackish mass, partly puriform, partly ichorous. From the jugular foramen onwards only an unconnected ragged mass could be traced; the portion of the jugular vein as far as the opening of the pharyngeal was completely destroyed, and pus poured out into the surrounding cellular tissue. Lower down, the jugular was again obstructed by a plug, which was soft and puriform in its upper part, but firmer below where it extended into the subclavian. Old disease of the knee-joint. Blood for the most part imperfectly coagulated; hyperæmia of the bronchi and lungs, and œdema of the latter. In the upper part of the right lung a dark-red deposit. Liver and spleen enlarged, the latter flaccid. Kidneys large and jaundiced. Hyperæmia and ecchymoses of the mucous membrane of the intestines; enlargement of the solitary glands.

[1] Virchow, 'Dessen Archiv,' 8, 375.

Although in this case the new growth in the petrous bone had probably produced a simultaneous caries of that bone, as Lebert[1] also assumes, it is certain that the narrowing of the channel by the tumour, and the contact of the blood with it, contributed to the production of the thrombus.

Case 34.—Paletta[2] gives an observation, also met with in Stannius, according to which the superior longitudinal sinus was so compressed by ulcerating tubercles (?) that its calibre, to the extent of three fingers' breadth, was entirely impermeable.

Förster[3] states that in a case of perforation of the sinuses from enlargement of the Pacchionian glands thrombosis existed in them, a phenomenon which Virchow,[4] however, has not observed.

Case 35.[5]—A soldier, æt. 18, was wounded in the left temporal bone by a hand-grenade. After he had been trephined feverish pyæmic symptoms, with cough, presented themselves. There was delirium, and death occurred on the twenty-first day.

Sectio.—On opening the skull it was found that a splinter of the vitreous plate half an inch long, had penetrated the superior longitudinal sinus. In the sinus was found puriform and polypous clot, of an atheromatous character, a portion of which obstructed the opening into the horizontal sinus. There were also pus beneath the pericranium, purulent arachnitis, and extravasation of blood upon the inner surface of the temporal bone.

In this case we might assume either a hæmorrhagic thrombosis, whether originating at the seat of injury in the bone or at the wound produced by the operation, or a caries of the bones of the skull, as the starting-point of the affection of the sinus; but the penetration of the splinter into the sinus is so manifest an origin of the coagulation of the blood that it is unnecessary to seek for any more remote one. The narrowing of the channel, on the one hand, and the contact with the foreign body on the other, may both have

[1] Loc. citat., 421.

[2] 'Exercitat. Patholog. Mediol.,' 1820, p. 94. Stannius, loc. citat., 36.

[3] 'Handbuch der Spec. Pathol. Anat.,' 616.

[4] 'Handb. d. Spec. Pathol.,' i, 164.

[5] Schmucker, loc. citat., i, 85.

contributed to bring about this result. Lastly, it may be remarked here that Tonnelé (compare Rilliet et Barthez, 'Maladies des Enfants,' i, 106) has observed swelling of tuberculous lymphatic glands and a tumour caused by caries of the cervical vertebræ, which, through pressure upon the vena cava, caused coagulation of the blood in it, and thrombosis which extended into the sinuses.

III. *Thrombosis of the Sinuses from debilitating influences.*

The number of the observations which come under this head amounts to nearly one fourth of all the cases collected by me. A closer examination of them furnishes numerous points of interest, especially in comparison with those grouped together in the first section. I prefix a brief description of the cases themselves.

Case 36.[1]—An old woman from the division of the so-called "Gâteuses" in the Sulpétrière, who was of weak intellect, died after twenty-four hours' coma.

Sectio.—A brilliant, black, adherent thrombus was observed in the superior longitudinal sinus; similar plugs filled the veins communicating with it. The gray substance of the greater hemispheres contained numerous capillary hæmorrhages; on the convex surface of the right hemisphere an extensive yellowish cicatrix existed, which had obliterated several of its convolutions.

Case 37.[2]—In a woman, æt. 80, a painless infiltration of the left lower extremity occurred, which was soon followed by paralysis of the left half of the body, with the exception of the face and tongue; the paralysed arm was occasionally the seat of contraction. Finally, paralysis of the right side and death.

Sectio.—In the anterior portion of the superior longitudinal sinus, an adherent thrombus, not discoloured, in the posterior portion one much changed in colour, extending on both sides an inch beyond the torcular Herophili into the lateral sinuses. The superior cerebral veins opening into it are also filled with plugs. In the sac of the arachnoid a fresh extravasation spreading over the convex surface of both hemispheres. Œdema of the pia mater; atrophy of the brain; extensive red softening of the gray substance of the

[1] Cruveilhier, 'Anat. Patholog.,' liv. 36, p. 2.
[2] Cruveilhier, loc. citat., liv. 36, pp. 4 and 5.

right hemisphere of the eerebrum; old, yellowish-coloured cicatrices and yellow softening of spots in the neighbourhood of the fissure of Sylvius. There was also purulent phlebitis of the common iliac and left erural veins, and the vena azygos contained pus almost as far as its communieation with the superior eava. Slight serous effusion into the left pleura. Lungs healthy. Partial atrophy of the parietal bones.

In both these cases the thrombosis of the sinuses is doubtless to be attributed to the great age of the subjects, and to senile marasmus. In the last, plugs in a state of suppuration existed in other parts of the venous system, whieh depended upon the same causes and appeared to be of earlier origin, as is shown as well by the history of the ease as by the more advaneed stage of metamorphosis.

Case 38.[1]—A ehild, two years of age, which had long been very weakly, died suddenly with symptoms of suffocation. At the post-mortem examination, a fresh elot was found in the anterior portion of the superior longitudinal sinus, while the posterior portion of the sinus was filled with false membranes and a fluid resembling the lees of wine; the veins eommunieating with the sinus were much distended with blood. In the eentre of the right hemisphere there was a large apopleetie elot.

In this case infantile marasmus would appear to have given rise to the thrombosis of the sinuses, and the case itself may fairly be grouped with the two preceding ones of marasmus senilis.

Case 39.[2]—A boy, fourteen years old, suffered from ague-caehexia, with œdema of the limbs and enlargement of the liver and spleen. Diarrhœa and eough eame on, followed by fainting fits, eyanosis of the faee, dyspnœa, and death.

Sectio.—In the superior longitudinal and lateral sinuses firm clots. Brain normal, with a large quantity of serum in the ventrieles. The spleen enlarged; the lungs hepatized.

Case 40.[3]—A boy, fifteen years old, who had been suffering for

[1] Tonnelé, loe. citat. Lebert, loe. eitat., p. 387.
[2] Tonnelé, 'Rust's Magaz.,' 41, p. 139, 1834. V. Albers.
[3] Tonnelé, loe. eitat.

several months from pleurisy, with œdema of the feet and dyspnœa, died suddenly.

Sectio.—In the superior longitudinal sinus there was a coagulum of considerable size, which contained yellowish pus. The vessels of the pia mater were filled with blood. Œdema of the pia mater and effusion into the lateral ventricles. Pleuritic exudation on the left side, with contraction of the chest. At the base of the left lung a cavity containing pus (metastatic abscess?)

Case 41.[1]—A little girl, four years old, who suffered from ophthalmia and swelling of the glands, was seized with pneumonia of the upper lobe of the right lung. She had swelling of the submaxillary gland and dilatation of the pupils. Death occurred on the fifth day.

Sectio.—In the superior longitudinal sinus was a clot which was closely adherent to the coats of the sinus, and partly in a state of purulent softening in its interior. This clot extended for some distance into *both lateral sinuses.* All the cerebral veins opening into the sinuses contained pus mixed with clots. An ecchymosis was visible on the left hemisphere. The brain could not be examined more minutely.

Case 42.[2]—A man, fifty-three years old, had cough with copious expectoration. Tenderness of abdomen; ascites; fever; anorexia; constipation. Urine scanty. Later on, pain in the neighbourhood of the liver; jaundice; increased ascites; *loss of strength;* diarrhœa; involuntary evacuations; accessions of unconsciousness; death.

Sectio.—In the longitudinal sinus a tolerably firm but completely discoloured plug, ¼ inch in length, occupying half the area of the sinus. This plug was a prolongation from one of the superficial veins of the arachnoid on the left side, which led to a mass of granulations. On the surface of the arachnoid a thin layer of exudation, with numerous fresh blood-corpuscles and yellowish icteric serum. In the granulations upon the whole surface, laminated chalky masses; the clots upon the granulations themselves partly knotty. Beneath the arachnoid of the cerebellum a small chole-steatoma. Ascites; cirrhosis of the liver; spleen small.

[1] Cruveilhier, loc. citat., l. 8, pl. 4, fig. 2 and 3, p. 4.
[2] Virchow, in 'Dessen Arch.,' viii, p. 376.

Degeneration of the kidneys with cysts and concretions. Œdema of the lungs, and obstructive plugs in the pulmonary arteries.

The cases 39, 40, 41, and 42 are instances in which the thrombosis of the sinuses must be regarded as the final result of chronic cachexia. But cases are not wanting in which quickly operating and highly debilitating influences lead to thrombosis of the sinuses, as will be shown in the following examples. Profuse hæmorrhages and fluid discharges are of especial importance in this respect.

Case 43.[1]—A woman, aged twenty-three, was twice attacked by peritonitis in the first week after delivery, for the cure of which repeated copious abstractions of blood were made during nine days. Twice, namely, forty leeches were applied, and twice twenty, or 120 altogether. A fortnight after delivery, headache and vomiting supervened, followed by hemiplegia. Great restlessness and screaming; coma and, finally, death three weeks after delivery. It must be added that, in consequence of these new symptoms, the patient was bled once from the arm, and fifteen leeches applied on various occasions.

Sectio.—The superior longitudinal sinus was seen very much distended, and of a glittering blackish appearance. It was filled by a clot, in the centre of which was a puriform fluid, resembling wine lees. The right lateral sinus, which was double at its commencement, also contained a similar fluid, blocked up in the direction of the jugular vein by a firm thrombus. The other sinuses quite normal. There were ecchymoses in the gray matter on the surface of the brain, especially in the course of the inflamed (thrombosed) veins on the convex surface and base. In the true pelvis, two deposits of pus; in the veins of the uterus, corresponding to the place of attachment of the placenta, very firm, small, black plugs. In the cavity of the chest nothing abnormal.

This case is certainly worthy of remark. That in childbed coagulations in the veins are of frequent occurrence is well known, but these are observed chiefly in the uterine veins, the plexus pampiniformis (ovarian veins) or the lower extremities, and their causation may generally be referred to a hæmorrhagic or mechanical origin, such as compression or dilatation. But thrombosis from marasmus

[1] Cruveilhier, 'Anat. Path.,' liv. 36, pp. 2 and 3.

during childbed is also not infrequent, since the act of parturition, and the loss of blood frequently accompanying it, are highly debilitating influences. It is true that, in the case mentioned above, the labour was natural; but the idea will obtrude itself, that the colossal abstractions of blood, worthy of even a Bouillaud, made on account of the peritonitis, gave the impulse to the thrombosis of the sinus, as it was shown in the earlier part of this paper that any considerable diminution in the general quantity of the blood may lessen the rapidity of the current and give rise to coagulations in the sinuses. There were, indeed, thrombi in the uterine veins, but these could not well be brought into direct relation to the formation of thrombi in the sinuses.

Förster,[1] who, besides weakness, regards excess of fibrin in the blood and increased coagulability as essential elements in puerperal thrombosis, appears also to have observed cases in which the coagulation occurred secondarily as a consequence of metastatic infarcta and points of inflammation, since the stagnation in the capillaries causes coagulations in the cerebral veins which extend into the sinuses. That the present case is not one of this kind appears tolerably clear, since, with the exception of some superficial ecchymoses, which certainly were rather the result than the cause of the thrombosis, no mention is made of hæmorrhagic infarcta, and metastases were nowhere observed, especially not in the lungs.

With this case another I am now about to communicate appears to have affinity, although other causes also seem to have been in action.

Case 44.[2] A soldier received a gunshot wound of the left parietal bone, which produced fracture with depression. The symptoms which supervened rendered trephining necessary, while the patient was bled five times in a short space of time. Death on the thirteenth day.

Sectio.—Fleshy coagula were found in the left lateral sinus; smaller similar ones in the superior longitudinal and right lateral sinus. There were also fracture of the inner plate and purulent arachnitis. In this case several explanations are possible for the formation of coagula in the sinuses. The injury itself, the opera-

[1] 'Handb. d. Pathol. Anatomie,' ii, p. 616.
[2] Schmucker, loc. citat., i, p. 75.

tion for trephining, and, lastly, the five bleedings within a short period, belong certainly to the debilitating causes which contributed their quota to the thrombosis, although the probability of a traumatico-hæmorrhagic thrombosis cannot be disputed, and these elements may all have exercised their influence simultaneously. If the last mentioned had been the cause, however, we should rather have expected to find a thrombus already breaking down.

CASE 45.[1]—A servant girl, æt. 20, who had been ill a week, was suffering on admission into the hospital from diarrhœa, with a red and rather dry tongue. She answered only briefly and slowly. In twelve days, symptoms of pleuro-pneumonia of the right side showed themselves. Other cerebral symptoms, such as contractions of the muscles of the neck, and afterwards of all the muscles, epileptiform convulsions of the right side, and coma, supervened; and death ensued on the 22d day after her admission into the hospital.

Sectio.—In the superior longitudinal sinus a firm, adherent thrombus, which was partly in a state of purulent decomposition in its interior; in the cerebral veins, black, firm, adherent plugs, discoloured at some points; and in the lateral sinuses adherent, buffy coagula. On the surface of the left hemisphere of the cerebrum, in the vicinity of the longitudinal sinus, capillary apoplexy and red softening, extending partly into the medullary substance. On the right side, the red softening was less extensive and less advanced. The lower lobe of the right lung was in the second stage of hepatization. About a foot above the cæcal valve, there were three round (ring-shaped? circulaires) ulcers. The mesenteric glands swollen, of a livid red colour, but not softened. Whether we have here to do with a case of typhus with secondary pneumonia cannot be clearly proved, on account of the insufficient description of the post-mortem appearances, but this assumption appears plausible. The thrombosis in the sinuses, which developed itself with its consequences after the occurrence of the pneumonia, may fairly be regarded as resulting from marasmus, unless we regard the cerebral affection as metastatic, and the thrombosis as a continuation; an assumption, however, which appears to me much more forced. The following case is exactly similar.

[1] Cruveilhier, loc. citat., liv. 20, pl. iv, p. 4.

Case 46.[1]—A girl, twelve years old, had been suffering for a month with feverish symptoms, anorexia, profuse diarrhœa, &c. When admitted into the hospital, her case was looked upon as an advanced stage of typhus. The diarrhœa continued, and her condition at the end of thirteen days was little improved. Clonic and tonic convulsions, with loss of consciousness, but not of sensibility, suddenly supervened. These phenomena lasted an hour, after which the patient became quiet. A fresh accession soon occurred, however, with trembling of the muscles, which lasted till death. The convulsions were followed by coma, with contraction of the fore-arms and hands, and contracted pupils; pulse 100. Death.

Sectio.—There were found typhous ulcers, partly cicatrized, partly undergoing the healing process, and small spots of purpura on the mucous membrane of the stomach and pelvis of the kidneys; similar small ecchymoses beneath the skin of the lower extremities, formed in the last hours of life. The whole of the longitudinal sinus was obstructed by a coagulum everywhere adherent and partly discoloured, as also all the veins of the pia mater communicating with it. The other sinuses were unchanged. The whole of the pia mater on the surface of the brain and between its convolutions replete with blood, and in some places, especially in the right Sylvian fissure, more considerable extravasations existed. In the right anterior lobe, a cavity filled with blood, partly recent, partly of longer standing.

To these eleven cases I add four others communicated by Dr. Gerhardt. They all occurred in very young, artificially fed children, and agree very closely with each other.

Case 47.[2]—A little girl, eight years old, fed artificially, and very emaciated, was seized with diarrhœa accompanied by cough, with symptoms of bronchitis and consolidation of the pulmonary tissue. The integuments of the head loose, the greater fontanelle small and sunken, the bones of the skull overlapping each other. Cyanosis; the vessels between the greater fontanelle and the temple much gorged; the right external jugular vein very little, the left very much distended. Occasional screaming; contraction of the muscles of the neck; unconsciousness; death.

[1] Bouchut, 'Gaz. des Hôpitaux,' No. 126, 1857. 'Canst. Jahresbericht,' pro 1857, iii, 222.

[2] 'Deutsche Klinik,' 1857, No. 45.

Sectio.—The bones of the skull overlapped each other, especially at the back of the head; a large quantity of serum flowed from the posterior cranial fossa. The superior longitudinal and right lateral sinus, with many communicating veins of the right hemisphere, were filled with firm thrombi, which were partly coloured red by recent deposit, but otherwise of a gray colour and hard, and in the longitudinal sinus adherent. In their interior they are partly softened. The pia mater much injected and œdematous. Œdema and great hyperæmia of the brain. The lungs, which are emphysematous anteriorly, show partial atelectasis and patches of lobular pneumonia. Melanotic discoloration of the small intestine and of the enlarged follicles in the large intestine.

Case 48.—A well-fed boy, three months old, was seized with profuse diarrhœa. The greater fontanelle flat and pulsating strongly. Temporal and frontal vein strongly developed. Jugular vein on both sides much and equally distended. The child lies quietly, with an unconscious stare and occasional strabismus; both pupils equally dilated. Complete unconsciousness, with opisthotonos and rigidity of the muscles, followed. The fontanelle sinks in, the bones of the skull overlap each other. Convergent strabismus; somnolence. First, the left external jugular vein became more distended, then the same vein on the right side enormously so, while that of the left side appeared almost empty; slight paralysis of the left side of the face; left pupil more dilated. After some transient improvement the child again became worse, with indications of consolidation of the posterior part of the right lung. Death on the 11th day.

Sectio.—Extravasation into the subcutaneous cellular tissue at the back part of the head; the posterior half of the bones of the skull very hyperæmic. In the superior longitudinal sinus anteriorly, fluid blood and recent coagula; an inch and a half from its posterior extremity commences a knobby, discoloured, but not yet softened thrombus; which extends of so great size into both lateral sinuses that these, especially the left, appear externally like thick, roundish, hard cords. They extend to within half an inch from the jugular foramen, and end in smooth points. The left is partially adherent, and completely fills the sinus; the right, not. Hyperæmia of the pia mater and gray substance of the brain. Patches of pneumonia in lower lobe of both lungs. Usual condition of intestines.

CASE 49.—A little girl, eleven years of age, sank after three days vomiting and diarrhœa. The bones of the skull overlapped each other. Sopor. Contraction of the muscles in the nape of the neck. Death.

Sectio.—Cyanosis of the nose as far as its root, of a portion of the cheeks, of the upper lip and eyelids. The anterior facial vein on both sides fuller of blood than the posterior and lingual, but contains no thrombi. The greater fontanelle much sunk in, the bones of the skull overlapping each other. Air being blown into the right internal jugular vein, the fontanelle stands out and the overlapping of the bones disappears almost entirely. In the straight sinus and first half inch of both lateral sinuses is a crumbling, adherent, symmetrically formed plug, quite discoloured, and completely obstructing the canal. Hyperæmia and œdema of the brain and pia mater, with the plexuses. Both lungs present a considerable extent of atelectasis. Catarrh of intestinal canal. Spleen enlarged. Small extravasations and gelatinous thickenings at the free edges of the mitral and triscuspid valves.

CASE 50.—A boy, three weeks old, was seized with diarrhœa and vomiting. Fontanelle depressed. Collapse, sopor, strabismus, paralysis of the right facial nerve. Contraction of the muscles of the nape of the neck and extremities. Right jugular vein somewhat less full than left. Later on, pulselessness and cyanosis of the face; the fontanelle fills out again and becomes tense; both jugulars equally imperfectly filled. Superficial veins of right ear much distended. Death in three days.

Sectio.—Body shows signs of marasmus. Fontanelle and sutures moderately open and tense. In the superior longitudinal sinus a very abundant blackish-red coagulum, partially adherent, extending for some distance into the lateral sinuses. Pia mater and brain anæmic, the substance of the latter soft. In the right hemisphere a cavity occupying nearly its whole extent, with ragged walls, which contained a considerable bloody mass, and extended slightly to the left hemisphere. At the base of the cerebellum rather considerable extravasations in the course of the vessels of the pia mater. In the veins of that neighbourhood, at different points, black, rather firm, coagula. In the mitral and tricuspid valves abundant gelatinous thickenings; upon one fold of the latter, and in the outer coat of the ascending aorta, slight recent extravasation. Lungs inflated,

somewhat œdematous; spleen small. Lithic acid infarcta in kidneys. Catarrh of intestinal canal.

The mode of origin of thrombi and the interpretation of the symptoms observed during life are very plausibly given in the excellent treatise of Gerhardt, which has, on the whole, my full concurrence. The profuse fluid discharges resulting from diarrhœa and cholera in infants necessarily diminish the quantity of the blood, and produce inspissation of it. Absorption of the parenchymatous fluid of the organs first occurs, especially in the very watery brain of the infant. The consequence of this is diminished volume of the contents of the skull, whereby the atmospheric pressure causes, first, depression of the fontanelles, and afterwards, when this no longer suffices, overlapping at the sutures. If this compensation also prove insufficient, a distension of the vessels of the brain and sinuses occurs, and the diminution of the general mass of the blood, the lowering of the power of the heart, and the inspissation of the blood retard the current, and lead to the formation of thrombus in a locality so favorable as the sinuses of the brain. The unequal filling of the jugular veins is a natural consequence of thrombosis of the lateral sinus on one side, inasmuch as the vein, the sinus supplying which is stopped, will be least full. This phenomenon, which must be most strongly marked in the deep-lying internal jugular vein, also shows itself by a circuitous route in the external jugular of the same side.

With this species of thrombosis of the sinus from marasmus may doubtless be associated the case given in the beginning of this paper. There also the extensive discharging abscess in the subcutaneous cellular tissue produced a profuse loss of fluids, and there also the subject is an infant, the impaired power of whose heart no longer sufficed to propel the diminished quantity of thickened blood through the sinuses of the dura mater with such rapidity as to prevent its spontaneous coagulation. From the more advanced consolidation of the bones of the skull, and the limitation of the thrombus to the superior longitudinal sinus, there could naturally be no overlapping of the bones nor an unequal filling of the jugular veins observed. The only phenomenon during life attributable to the obstruction of the superior longitudinal sinus is the epistaxis which occurred shortly before death. It appears to have resulted from the impediment to the flow of blood in those veins of the nose which pour their blood into the superior longitudinal sinus through

the foramen cæcum, and are generally larger in children than in adults. Moreover, with the exception of a transient attack of convulsions, any symptoms indicative of an affection of the brain were wanting; and if we bear in mind how frequently convulsions occur in the diseases of early infancy, we cannot attach much importance to their occurrence in this case.

To assume a direct metastatic source of the thrombus in the sinus from the seat of the abscess in the thigh, appears to me in the highest degree unreasonable, since any emboli detached thence would have to pass two systems of capillaries before reaching the sinus. I am unable to state in what relation the recent endocarditis and lobular pneumonia, which did not present the metastatic but the lobular form peculiar to early age, stood to the sanious abscess.

If we compare these cases of thrombosis of the sinuses from marasmus with those in which the thrombosis depended upon processes of inflammation, a series of differences very essential in themselves and very important for the diagnosis of future cases presents itself.

First, as regards the age of the patients in the two classes, we are at once struck with the circumstance that while, in the first class, the middle period of life, between the time of the development of puberty and old age, predominates, in the second it is especially the period of childhood up to the 14th year which is represented, with the addition of a few cases at a very advanced age. If to this we add that Rilliet and Barthez[1] observed eighteen cases of thrombosis of the sinuses in children which belong to the last class, a comparatively very great frequency of thrombosis of the sinuses from marasmus in infancy becomes apparent.[2]

[1] 'Traité des Maladies des Enfants,' 2ème édit., i, 166.

[2] The above-named authors give the following table of age for their cases:

Children of 2 years old . . .	6
" 4 " . . .	4
" 5 " . . .	1
" 6 and 7 years old . .	2
" 9 years old . . .	2
" 10, 11, and 14 years old . .	3
	18 cases.

For the sixteen cases reported above (including mine) the following summary presents itself:

In all these cases it is distinctly mentioned that a state of chronic debility existed (les enfants étaient minés par des affections chroniques); a large number of them were tuberculous, others rachitic, and others again so debilitated by acute diseases that their very convalescence had the appearance of a second disease. The origin of the thrombosis in marasmus in these cases thus appears established beyond doubt. The small power of the circulation and respiration in childhood, and the more frequent occurrence of caries of the bones of the skull in adults, sufficiently account for this fact. A further circumstance well worth remarking in reference to the ætiology of the cases given in the second series presents itself in the fact, that in a comparatively large number of them *obstructions of the respiration* were observed which must have prevented the right side of the heart from emptying itself properly, and retarded the current of the blood in the veins of the neck and sinuses. Friedreich[1] has already called attention to this fact when speaking of Gerhardt's cases, and insisted upon its importance as a further cause of retardation of the current of the blood. Independently of the feebleness of the muscles of respiration in childhood, which impedes the flow of blood to the chest, we find in ten out of our sixteen cases special impediments mentioned, consisting in affections of the organs of respiration, such as lobar and lobular pneumonia (in children), with extensive atelectasis of the lungs, chronic pleuritic exudation with contraction of the chest, and plugs in the pulmonary arteries with œdema of the lungs in case 42. In this case it may, indeed, be doubted whether these plugs are not to be regarded as a

Children under 1 year	5
,, of 2, 4, and 5 years	3
,, of 12—14 years	2
,, of 20 and 23 years	2
An adult	1
A man æt. 53	1
An aged woman	1
A woman æt. 80	1

Thus 10 cases occurred during childhood, 4 during the middle period of life, and 2 in old age.

Of 28 cases of thrombosis of the sinuses in consequence of processes of inflammation, in which the age is stated, 6 occurred before the fourteenth year, of which only one occurred in the first year of life; and 22 after the fourteenth year, of which not one occurred later than the fifty-fourth year of life.

[1] Canstatt, 'Jahresbericht' pro 1857, iii, p. 223.

consequence of the thrombosis of the sinuses, but Virchow makes no mention of their embolic character, nor were any changes met with in the lungs corresponding to such an origin. But the ascites occurring simultaneously could not fail to impede the respiration, while the very painful peritonitis which preceded the thrombosis in case 43 must have checked the energy of the respiratory movements for a considerable time. In most of the cases observed by Killiett and Barthez, also, impediments to respiration existed, such as tuberculosis of the lungs and rachitis, in the latter of which the muscular debility and the projections of the chest present themselves as such.

It is quite otherwise, however, with the affections of the respiratory organs not unfrequent in the cases in the first section. They consist chiefly in metastatic deposits in the lungs, or in metastatic pleurisy, and are thus to be regarded as consequences of the thrombosis, not as causal elements in its production.

It seems also to be something more than mere accident that essential deviations as regards the site of the thrombosis in the two series of cases exist. I have already remarked that in thrombosis of the sinuses, resulting from caries of the petrous bone, the site of the thrombosis, except in a single case in which it occurred in the cavernous sinus, was, for obvious reasons, in the lateral or superior petrosal sinus of the side affected, whence it frequently extended to the cavernous sinus and internal jugular vein. Once only did it extend as far as the posterior extremity of the superior longitudinal sinus and lateral sinus of the opposite side. *The extension*, therefore, is mostly *confined to one side*, and affects *duplicate sinuses* and *veins* of one or the other side. We also observe that in the other cases of this series the thrombosis almost always commenced in the sinus nearest to the caries of the bones of the skull, the seat of injury, or other inflammatory affection, so that the anatomical connexion with the part first affected is evident. On the whole, the azygous superior longitudinal sinus was the seat of thrombi *only when* either the thrombosis was very extensive, or inflammation and injury existed within the area of the veins of the pia mater, dura mater, or parietal bone, which discharge themselves into it, as follows from the assumption that the thrombosis is to be regarded as a prolongation from the smaller veins into the sinus.

In the second series we see, on the contrary, that thrombosis of this azygous sinus is wanting *only once*, and that in a case in which another azygous sinus—the straight sinus (case 49)—was filled with

clot. In six cases the superior longitudinal sinus *alone* was the seat of the thrombosis, in seven it extended from the superior longitudinal sinus into both lateral sinuses, in one from the straight sinus into both the lateral. In two cases only were the superior longitudinal and right lateral sinus alone thrombosed. The thrombi present, for the most part, a symmetrical form. This phenomenon plainly indicates that the cause of the thrombosis in these cases was not a local but a general one, the influence of which affected equally both sides of the skull, and extended, by preference, symmetrically from the azygous superior longitudinal or straight sinus beyond the torcular herophili into both lateral sinuses, or was confined to the superior longitudinal sinus alone.

The character of the thrombi also differs considerably in the two series. In the first series, most of the thrombi (in twenty-six cases out of thirty-two) had already undergone rather advanced changes; and, as regards their condition in cases of caries of the petrous bone, I refer my readers to what I have already stated in the appropriate place, from which it appears that in four cases only was the thrombus firm, more or less discoloured, and not exhibiting in its interior any signs of softening and molecular decay. In the remaining twelve cases of the first section it is seen, with the exception of cases 31 and 32, in which more minute details are wanting, that the thrombi are always more or less broken down already, and either changed completely into a puriform or even sanious mass, or consist of pseudo-membranous coagula mixed with puriform matter. Amongst the cases of thrombosis which I have grouped together as resulting from marasmus, it is seen, on the contrary, that in eleven cases the thrombus was not yet discoloured, or, if discoloured, still of firm, at the most crumbling consistence; and that in five cases only the plug was softened in its interior, where it contained puriform fluid resembling wine-lees in colour, while these changes never reached the periphery of the thrombus, and the walls of the sinus remained unaltered. On the other hand, roughness of the inner coat, thickening of the walls, and discoloration, or even, as is distinctly stated in some cases, ulceration of them, are frequently mentioned in the cases of the first section. This difference in the character of the thrombus must either result from the longer or shorter time required for its formation, or from some original difference in its nature. Both seem to be the case. In thrombus from marasmus the process is effected, as one of the last results of previous

debilitating diseases, shortly before death, which it tends to accelerate. In the thrombosis which results from inflammatory and sloughing processes, on the contrary, the thrombus frequently contributes less directly to death, and being of slower growth, exists for a considerable time during life, while, from the imbibition of purulent and sanious matter, it has a greater disposition to break down, and is, consequently, found in a more advanced stage of metamorphosis in the dead body.

It still remains to point out the difference observable in the two series in reference to the pathologico-anatomical changes in the brain and its membranes. While, in the cases of the first series, purulent and sloughing inflammations of the arachnoid and dura and pia mater, with formation of abscesses in the brain (in thirty-two cases nineteen times, or 60 per cent.), occur, which, in the minority, must be regarded as causal elements in the production of the thrombi, in the majority as co-effect of the same cause, viz., of the caries, &c., it has already been remarked that effusions of blood upon the membranes of the brain in caries of the petrous bone are only twice mentioned, and then only to a slight extent, while extravasations into the substance of the brain did not occur at all. And in the other twelve cases of this group we only three times find mention of extravasations, of which one (No. 24) in the substance of the brain must be regarded rather as the immediate result of external violence; the two others (28 and 29) had their seat in the arachnoid, upon the left hemisphere and upon the medulla oblongata, and were inconsiderable. In the thirty-two cases, therefore, there are only four (12·5 per cent.) in which extravasations existed which could be regarded as consequences of the thrombosis. In the second series, on the contrary, we but seldom observe inflammations of the brain and its membranes; in No. 42, a thin layer of exudation mingled with blood-corpuscles upon the arachnoid, and, in No. 44, purulent arachnitis, *i. e.* twice altogether (12 per cent.), whilst the extravasations, on the other hand, are much more frequent and extensive. In eight cases out of sixteen (50 per cent.), effusions of blood, for the most part considerable, were found; twice, large apoplectic clots in the brain; twice, extensive extravasations in the arachnoid; and several times very extensive capillary hæmorrhages and red softening of the substance of the brain. If we inquire into the cause of this difference in an anatomical point of view, the explanation of it as regards the first point

(inflammatory affections of the brain and its membranes) lies so near at hand as to require no further allusion. As regards the second point (hæmorrhages), it appears to require further elucidation, since hæmorrhages from the complete obstruction of one of the larger trunks, such as the sinuses, and rupture of vessels in the soft and yielding tissues, must be as frequent in the one case as in the other. It appears to me that the different mode of origin of the thrombi may clear up this point. I have already attempted to establish the view, for the majority of the cases in the first series, that the thrombosis does not generally originate in the sinus itself, but must be regarded as a continuation from the small veins of the diploë or other inflamed parts. In this way the thrombus is formed more gradually, advancing along the walls of the sinus, and not occupying its whole calibre till some time afterwards, so that collateral veins continue for a while to pour their blood into still unobstructed portions of it; the distension of the communicating veins will, therefore, have time to spread further, and find other, though insufficient, outlets. In thrombosis dependent upon marasmus, the thrombus generally arises first in the sinus itself, because the conditions there are most favorable to its formation. The blood coagulates more quickly because its movement is slower, the coagulation frequently extends to all the veins opening into it, and the calibre of the sinus sooner becomes completely obstructed, before a collateral circulation is properly established and the blood distributed elsewhere. The distension and tenseness of the communicating vessels will therefore reach a high point in a shorter time, which explains the greater tendency to ruptures and hæmorrhages. In addition to this, it is almost always the azygous superior longitudinal sinus (which lies in the median line, and receives blood from both hemispheres) which is found obstructed in the cases of the second series, and a collateral compensation of the excess in the veins opening into it must be less easily established than when merely one or other of the duplicate sinuses has become impermeable.

From what has just been said it also follows that venous hyperæmia and œdema in the brain and its membranes must be frequent post-mortem phenomena in both series, as is found to be really the case.

In conclusion, it is worthy of special remark that a difference exists with regard to the *frequency of metastatic processes* in the

cases of the two series. In the first they are very frequent (sixteen times in thirty-two cases), while in the second they are altogether wanting. Since, to explain the establishment of metastatic processes as they occur chiefly in the lungs, and more rarely in the liver and in other parts of the body, we are driven more and more to the assumption of embolus, we must look for the cause of this difference more particularly in the character of the thrombi; and this furnishes, in reality, a sufficient explanation of the fact. In the first series, the thrombi are generally continuations from the smaller veins, which, as Virchow has shown, offer the best opportunity for the separation of particles by the current of the blood; in addition to which, their greater tendency to decomposition favours such separation. In the second series, the more rapid mode of formation in the sinus itself, the firm character of the coagulum, and the retardation of the current of the blood from marasmus, are unfavorable to the separation of particles from the thrombus. It thus becomes evident why the so-called pyæmic symptoms, and the rigors followed by adynamic fever, occur in the cases of the first group, while they are altogether absent in those of the second. It now only remains to inquire whether any and, if so, what symptoms are peculiar to thrombus of the sinuses during life. In the majority of cases such symptoms are wanting, and we find only those proper to affections of the brain and its membranes—affections which, as we have seen, have either had a common origin with the thrombosis of the sinuses, or must be regarded as resulting from it. Neither can it be said that the thrombosis of the sinuses impresses any peculiar stamp upon these affections as regards their external phenomena by which to distinguish them from such as have resulted from other causes.

But the aggregate of the symptoms, as existing in Gerhardt's cases, may render it possible to diagnose thrombosis of the sinuses in children. If profuse diarrhœas in weakly children are followed by cerebral symptoms, with depression of the fontanelles and overlapping of the bones of the skull, accompanied by an unequal distension of the external jugular veins of the two sides, the diagnosis of thrombosis of the sinuses is a probable one, and that on the side on which the vein is least distended; but the absence of the last-named phenomenon by no means excludes the possibility of thrombosis of the sinuses, because, if it exist chiefly in the superior longitudinal sinus,

or in both lateral sinuses simultaneously, an unequal distension of the external jugulars cannot be observed.

In some few cases, indeed, distension of individual veins of the face is mentioned, which might be referred to the thrombosis of the sinuses, but cannot be employed as an aid to diagnosis, any more than the epistaxis in the case communicated by me.

It has already been remarked that, amongst the fifty-seven cases of thrombosis of the sinuses which furnished me with materials for this paper, there are six in which the production of the thrombus cannot be properly explained, either from want of a sufficiently detailed report or for other reasons. To complete the sketch, I will give them briefly here; and we shall find that in some of them, by help of the data furnished above, we may infer with great probability at least the mode of their causation.

Case 50.—Abercrombie[1] mentions an observation by Prichard, in which, in a woman who had suffered from epilepsy for two years, and who died in an epileptic fit not preceded by other symptoms, the left lateral sinus was filled by a coagulum which resembled organized lymph. How far the retardation of the circulation in the veins from the repeated epileptic attacks, or an unrecognised process of disease in the bone, may have led to the coagulation in the sinus, I cannot undertake to determine. I will only remark that epileptic attacks are mentioned in a case given by Puchelt,[2] in which caries of the petrous bone was the cause of the thrombosis.

Case 51.—Cruveilhier[3] gives the following post-mortem appearances, with a plate. In the superior longitudinal sinus of a girl, æt. 22, was found a dark, partly discoloured, very firm, and very adherent plug, which showed signs of puriform softening in some parts of its interior. The veins communicating with it contained stiff thrombi. Capillary apoplexy and yellow softening over a considerable extent of both hemispheres of the cerebrum.

From the character of the very firm thrombus, which was softened

[1] Loc. citat., p. 60; and Prichard, 'Diseases of the Nervous System,' 276.
[2] Loc. citat., ii, 177.
[3] 'Anat. Pathol.,' liv. 36, pl. 1, fig. 1.

in the interior only, from its seat in the superior longitudinal sinus, and from the hæmorrhages occurring simultaneously, this case may, with great probability, be assumed to be one of thrombosis from marasmus.

Cases 52 and 53.—In the same author,[1] two cases of Burnet's are mentioned, occurring in children; in one of which, the superior longitudinal sinus, the left lateral sinus, and the superior cerebral veins were the seat of black, adherent coagula. In the gray and white substance were numerous small coagula, some as large as peas, in the neighbourhood of which the brain was found to be softened and of an orange colour. There were similar patches in the right corpus striatum and left thalamus opticus. In the other case, the superior longitudinal sinus and superior cerebral veins were filled with adherent thrombi, while a number of small apoplectic clots were found in the substance of both hemispheres.[2] The early age of the subjects, the situation of the thrombus, and the extensive hæmorrhages, render it probable that both these cases of thrombosis also resulted from marasmus.

Case 54.—Gintrac[3] mentions a curious case of disease in a boy four years of age, who had suffered from birth from attacks of transient suspension of voluntary motion, with impaired sensibility, but without loss of consciousness. Death resulted from pneumonia. The superior longitudinal sinus was found to be converted into a hard cord; its walls were thickened, of a yellow colour, and so tough that they creaked under the knife. The sinus contained a firm clot, and the veins opening into it were gorged with coagulated blood.

The coagulum in the sinus and veins communicating with it was evidently of more recent origin than the thickening of its walls, which probably resulted from a previous affection (thrombosis?) of the sinus itself. What connexion existed between it and the peculiar attacks cannot be determined. The more recent thrombosis might perhaps be assumed to have a relation to the pneumonia.

[1] Loc. citat., liv. 8, pl. 4, p. 3. Remak.

[2] I, unfortunately, could not get access to the 'Journ. Hebd.' for April, 1830, from which these data are taken, and in which more minute details might perhaps be obtained.

[3] 'Recueil d'Observations,' 1830; and Andral, 'Clinique Méd.,' v, p. 277.

Case 55.—Ammon[1] relates an equally curious case of disease in a boy of fifteen, who was first seized with gastric symptoms. These were followed by headache, vomiting, loss of sight, distortion of the face, delirium, sopor, paralysis of the bladder, and subsequently of all the limbs, and finally by death. The coats of the veins in the dura mater were greatly thickened, as were also the walls of the superior longitudinal sinus, the calibre of which was diminished by a deposit of a gelatinous, whitish-yellow mass upon the inner coat. Hyperæmia and œdema of the membranes, with gelatinous exudation on the surface of the brain. Effusion into the lateral ventricles. General pulpy softening of the whole brain, especially of the thalami optici. The Pacchionian glands were enlarged.

Here, also, an earlier diseased condition of the sinus appears to have existed, in consequence of which, under circumstances which cannot indeed be explained, a general dropsy of the brain and its meninges was developed. A thrombus-formation, properly so called, appears, however, not to have taken place, unless we think proper to regard the whitish-yellow deposit upon the coat of the sinus as such.

If we once more sum up briefly the results of the foregoing investigations, we may express them as follows:

Thrombosis of the sinuses of the brain is either *a prolongation from the neighbouring veins*, or it *originates primarily in the sinus*.

A. *Thrombosis of the sinuses by prolongation* is the consequence—

I. *Of processes of inflammation* tending to necrosis and sanies in the vascular precincts of the sinus.

These consist chiefly in caries of the bones of the skull, caries of the petrous bone from internal otitis being most frequent.

II. *Of injuries of the bones of the skull,* inasmuch as the hæmorrhage from the diploë which follows them leads to coagulation (hæmorrhagic thrombosis).

III. *Of effusions of blood into the substance of the brain or its membranes,* from which the thrombus extends through the smaller veins into the sinuses (hæmorrhagic thrombosis).

This form of thrombosis is characterised by the situation of the thrombus in the sinus (generally azygous) nearest to the cause; by

[1] 'Médecin. Ann.,' viii, p. 208.

the more advanced softening of the thrombus; by changes in the walls of the sinus; by inflammation in the brain and its membranes; and by metastatic processes in other organs.

B. *Thrombosis originating primarily in the sinus* is the consequence—

I. *Of influences which retard the current of the blood.*

In the generality of cases several causes act simultaneously in this direction, partly of a general, partly of a local character.

1. *General causes retarding the current of the blood are*—

a. Deficient energy of the heart's action.

- *α. In advanced age* (*marasmus senilis*). Diminished elasticity of the coats of the arteries must here be taken into account as a favouring element.
- *β. In infancy* (*marasmus infantilis*).
- *γ. In consequence of precedent acute or chronic diseases.*

b. Diminution of the quantity of the blood. Its effect in retarding the circulation manifests itself chiefly in the sinuses of the brain. (It is generally associated with the causes mentioned under *a*).

- *α. Direct retardation from losses of blood.*
- *β. Indirect retardation from profuse secretions,* in which cases a certain allowance must be made for the inspissation of the blood (diarrhœa and cholera infantum, profuse suppuration).

c. Impediments to the expansion of the lungs, which prevent the right side of the heart from emptying itself properly. These impediments exist partly in the lungs themselves (pneumonia, atelectasis, tuberculosis), partly in the pleura (pleuritic effusion), or result from deficient action of the respiratory muscles (in rickets, ascites, peritonitis). Alone they do not appear to produce a thrombosis in the sinuses, but they must be regarded as very powerful auxiliaries.

The thrombosis which results from the causes mentioned under B, I, 1, acting for the most part in combination (thrombosis from marasmus), is characterised by the situation of the thrombus most frequently in one of the azygous sinuses (the superior longitudinal or straight sinus); by its firmness; by the non-existence of disease in the walls of the sinus; by consecutive hæmorrhages in the brain and its membranes; and by the absence or very unfrequent occurrence of metastatic processes in other organs.

2. *Local causes which retard the circulation in the sinuses are—*

a. Pressure upon the sinus itself by tumours and enlarged Pacchionian glands.

b. Pressure upon the large veins of the neck by tumours, in consequence of which coagulation occurs first in them, and by extension of the thrombus, also in the sinuses.

(This comes, properly speaking, under A).

c. Intrusion of foreign bodies and tumours into the sinus, which diminish its calibre; here contact of the blood with the foreign body must be taken into account as favouring coagulation.

II. *Of disease of the walls of the sinus,* from altered molecular attraction between the diseased walls and the blood, especially in inflammatory processes in the former (?).

After the foregoing paper was concluded, I noticed in No. 1 of the 'Oesterreich Zeitschrift für practische Heilkunde, 1859,' a case of thrombosis of the sinuses, communicated by Professor Pitha, which I cannot refrain from appending here, because it confirms in many respects the views advanced by me in that paper.

A hussar, æt. 26, received four sabre wounds on the head in a brawl. On receiving the last he fell, bleeding profusely, and was carried away unconscious. All the wounds were on the left side of the head, one on the frontal bone, one on the left parietal, one near the occipital protuberance, and one behind the left ear, running obliquely downwards and backwards over the mastoid process. The wounds on the frontal bone and mastoid process reached the bone in places. No fracture could be detected. There was great anæmia; pulse weak, 60. Partial somnolence and apathy, which disappeared after two days. The pulse sank to 48 and 45, while the patient's general condition was favorable, and the wounds healing, and suppurating slightly and healthily at those points only where the bone had been injured. On the 12th day, however, a thin greenish-yellow deposit was observed upon them, which disappeared in three days, after the application of nitrate of silver. During the night of the 27th day, severe pain came on in the left ear, with increase of temperature, and painful swelling of the glands in the neck; pulse 88. On the following day, sputa tinged with blood, and crepitation in left lung; wounds suppurating moderately, that behind the ear rather more copiously, and the sound shows a rough spot on the mastoid

process. On the 32d day severe rigors, followed by heat, pulse 112; on the following day, a return of the rigors, with symptoms of pneumonia on the left side. During the next few days all the symptoms abated somewhat; the sputa remained of a brownish-gray colour, and smelt very offensive. From the 39th day, great debility, typhous condition, enlargement of spleen, diarrhœa, delirium, involuntary evacuations, collapse, and jaundice. On the 45th day, the globe of the right eye was abnormally prominent and tense; pupil fixed, and moderately dilated; cornea dull; slight œdema of conjunctiva; complete loss of sight. On the following morning, a similar state of things in the left eye; the œdema of the conjunctiva increased, and spread to the orbital region and over the left temple, as far as the angle of the lower jaw. Death on the 46th day.

Sectio. Skin of a dirty yellow colour; all the wounds on the head healed except the fourth, which is still partly open, with livid edges. At the root of the mastoid process there is a loss of substance the size of a bean in the cortical layer, leaving a ragged surface. This part of the surface of the bone is covered with gray pus and small granular exudation; the bone in the vicinity rough as far as the mastoid foramen, the opening of which is covered with a layer of similar exudation. Skull, dura and pia mater normal, and containing a moderate quantity of blood. Brain perfectly normal. Upon that portion of the dura mater covering the left middle cranial fossa and clivus a thin layer of exudation, the hypophysis surrounded by ill-conditioned pus; the left lateral sinus, the petrosal sinuses, the circular sinus, and both cavernous sinuses gorged with yellowish-white pus, extending into the ophthalmic veins; the corresponding duplicate sinuses of the right side and both ophthalmic veins thrombosed, and only here and there containing thick pus. In the remaining sinuses, partly coagulated, partly fluid blood. Sloughing lobular pneumonia of both lungs; on the right side sanious pleurisy. Liver and spleen enlarged. Examination of the eyes showed complete thrombosis of the ophthalmic veins as far as the bulbs, without a trace of softening in the thrombi.

Pitha attributes the phlebitis and thrombosis to the entrance of pus from the external wound of the head through the strikingly large mastoid foramen into the lateral sinus,—a view which I cannot adopt, because it is not proved that the entrance of pus into the

blood causes coagulation of the latter, and because, with the possibility of the entrance of pus through the foramen into the sinus, a subsequent hæmorrhage from the latter must have occurred. Pitha, while taking into consideration the possibility of phlebitis in the diploë, thinks the assumption inadmissible, because the normal state of the wound is opposed to it. To this it might be objected that the condition of this wound during life was not altogether normal, and that the post-mortem examination showed superficial caries, inasmuch as a portion of the outer layer of the mastoid process was broken off, and the bone at this point covered with gray pus, and the bone in the vicinity as far as the mastoid foramen was rough. The possibility of phlebitis in the diploë, from which the thrombosis might spread to the sinus, cannot, therefore, well be denied.

The following explanation appears to me, however, more admissible. The sabre-cut behind the left ear injured the mastoid foramen, and thus, as Pitha also conjectures, caused the very considerable hæmorrhage. The thrombus which formed in the injured foramen extended gradually into the left lateral sinus, reached the left cavernous sinus through the petrosal, and the right cavernous sinus through the circular. This extensive and long-existing thrombosis in the sinuses also explains the continuance of the strikingly slow pulse. When the thrombus began to break down, fever set in, with pyæmic symptoms and metastatic deposits in the lung. That this thrombosis must have already existed for a long time is shown by the fully puriform state of decomposition in which the coagula were found. At the last only, fresh thrombi formed in the ophthalmic veins and remaining sinuses of the right side.

From the purely local cause of the thrombosis, the azygous sinuses, even the superior longitudinal, notwithstanding its great extent, remained free, and carried on the circulation within the skull. Here also, as in the majority of the cases which I have described in the first section, we find purulent exudation upon the dura mater and metastatic deposits in the lungs, with advanced decomposition of the clots, while, notwithstanding the great spread of the thrombi, no extravasations were found within the skull.

CASE

OF

ATROPHY

OF THE

LEFT HEMISPHERE OF THE BRAIN,

WITH

COEXISTENT ATROPHY OF THE RIGHT SIDE OF THE BODY.

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WITH ADDITIONAL NOTES BY THE AUTHOR.

THE NEW SYDENHAM SOCIETY,
LONDON.

MDCCCLXI.

CASE OF

ATROPHY OF THE LEFT HEMISPHERE OF THE BRAIN,

WITH

COEXISTENT ATROPHY OF THE RIGHT SIDE OF THE BODY.

Among the numerous lesions of the body met with in the course of anatomical investigations, those involving disturbances of the most important and noblest parts, and especially of the brain, are in many respects deserving of the greatest consideration. This is particularly true in reference to the close connexion between the brain and the rest of the body, which still presents so many unsolved inquiries to the physiologist.

Although it is generally known that the brain, as the seat of operation of our higher faculties and of volition, stands, by means of the spinal cord and nerves, in the most intimate relation to the rest of the body, disseminating through the entire system the orders of our will along the nerves as so many telegraphs, and receiving in return impressions from without, the varied influence of the cerebrum on the remainder of the organism is by no means limited to these functions; on the contrary, it extends far beyond them. Our body is, in fact, a whole, all the parts of which are not only most intimately connected with one another, but every separate part of which acts more or less upon the whole, and, according to the different extent of its sphere of action, contributes in a greater or less degree to the maintenance and regular working of the entire system. In this respect the nervous system occupies a first place, and its influence, not only

upon motion and sensation, but particularly on the nutrition of the body, is incontestably most important. The influence of the brain and of the spinal cord upon nutrition and the growth of the rest of the body is one of the most difficult subjects of investigation, and it is one upon which a great difference of opinion exists among physiologists.

It is generally admitted that the so-called vegetative part of the nervous system, or the sympathetic nerve, whose filaments, more or less mingled with the cerebral and spinal nerves, spread through the whole body, exercises a most important influence. It is, however, not so easy to determine what part the brain and spinal cord mediately or immediately contribute to this influence. It has, indeed, been proved beyond all doubt that long-continued paralysis leads to emaciation and atrophy of the paralysed parts; but whether this result is to be considered merely as secondary, and as depending on the continued rest and want of action caused by the paralysis, as most physiologists assume, or whether the brain and spinal cord may, by their close connexion with the vegetative portion of the nervous system, contribute more immediately to its production, is still, in my opinion, an undecided problem.

It is self-evident that neither vivisections of animals, which much too rapidly result in death, nor the observation of those cerebral lesions, the influence of which on the rest of the body is, on account of their speedy termination, of too short duration, can contribute anything to the solution of this important question. It is only such affections of the brain as are of sufficient extent and duration to manifest the want of the action of the organ on the other parts of the system, and still do not destroy life, so that their effects may work long enough upon the body, which are competent to add any light to the subject. That such lesions as interfere with a considerable portion of the brain while life still continues should be of rare occurrence cannot excite surprise, as usually a slight injury of the cerebrum is sufficient to cause speedy death; the wonder is, on the contrary, that life may last for many years where there is destruction, or at least severe injury, of one half of the organ.

Having had an opportunity, a few years ago, of dissecting a body in which nearly one half of the brain was softened, atrophied, and in a state of morbid degeneration, I thought that an accurate investigation of the evident influence of this cerebral lesion over the rest of the system would be the more important, as in the few instances

of this condition to be found in the works of modern writers the examinations made have been too superficial, and the details of the differences between the healthy and morbidly affected parts have not been traced with sufficient accuracy.

In the year 1844 I received from Amsterdam a body to be used at my anatomical lectures, and having been informed that it was that of an idiotic girl, I opened the skull with care. As to her previous history, I received the following information from my esteemed friend, Professor Schneevogt:

"Engeltje Noordman, aged 27, had from her earliest infancy been paralysed on the right side. Notwithstanding the poverty of her parents, and although she early exhibited all the evidences of imperfect mental development, the advice of the most eminent men had been obtained, who had treated her with both external and internal remedies, particularly with stimulants, until, in despair of her recovery, all further attempts were given up.

"In consequence of her infirmity, she usually kept her bed. At first, her imbecility was not so strongly marked; hence it was that she learned to speak and to make known her wants and manifested some human feeling. By degrees, probably from want of care, she began to grow stupid, exhibited less sympathy with the external world, became more violent in her desires, and passionate if they were not satisfied. In a word, she became more and more like an unclean beast. This was followed by a pruriginous eruption, which, together with a fit of mania, in which it was stated she had endeavoured to set the place on fire (although Professor Schneevogt, probably correctly, doubts the report), induced her mother to bring her to the Suburban Hospital at Amsterdam.

"She was then atrophied and paralysed on the one side, but not so much so as to prevent her moving her leg, which was usually drawn up; sensation was not impaired in the side, and, although it could not be clearly ascertained, to all appearance she saw equally well with both eyes, at least in their motions they preserved their parallel direction. Her mental development was at the lowest ebb; she took notice of nothing, except of the gratification of her animal wants; she made known her desires in this respect in broken language; she soiled herself without seeming to be aware of it, and sometimes, like a filthy ape, pelted others with her excrement. When her bedding was changed, or when she was washed, she raved and scolded in interrupted sounds. Menstruation was never

observed, nor did she ever exhibit any signs of sexual desire. Professor Schneevogt could not obtain any confirmation of his suspicion of the practice of onanism. She never manifested any indication of bodily suffering or oppression; she could lie on both sides, though she lay mostly on the right; her appetite was good, and her animal functions were normally performed. Although the motion of her face was not wholly destroyed, its right side generally hung paralysed and flaccid; and notwithstanding that her speech was very defective, and that she seldom expressed herself in words, making known her feelings rather by means of gestures and inarticulate sounds, she gave evidence of feeling in the right side. As she was in the hospital only from the 21st September to the 29th November, when she died, and on account of psora was kept among the scabious women, her case was only for a short time under observation."

At the post-mortem examination the remarkable emaciation and atrophy of the right side of the body at once struck the eye; in opening the skull much greater difficulty was experienced in sawing through the left half than the right, in consequence of the bones being much thicker on the former than on the opposite side.[1] On removing the upper part of the skull we were at once struck with the great difference in the circumference of the hemispheres of the brain as seen through the dura mater. This membrane having been removed, it appeared that the right hemisphere of the cerebrum was quite healthy; the pia mater was in this part coloured moderately red by the blood-vessels, the convolutions were natural, and the colour of the gray matter was, as usual, light red. The left hemisphere was, however, not only much smaller, but the arachnoid over the entire surface was very much thickened,[2] and between it and the pia mater a considerable quantity of fluid had collected. The whole hemisphere was very soft, and fluctuated like a bladder, on account of the fluid contained in the ventricles; the gray matter was not reddish, but very pale and yellow; the convolutions were much thinner than those of the other hemisphere, while the pia mater was very loosely attached to the surface, here and there, in fact, appearing to be separated from it by effused serum. The blood-vessels of the pia mater contained less blood than usual. On opening the ventricles, that on the left side appeared larger; the walls of the left ventricle

[1] Figs. I and II, *a, b, c*.
[2] Fig. III, *a, b, c*.

were much thinner, and, particularly at the posterior cornu, appeared extremely soft and nearly broken down.[1]

The right corpus striatum (Fig. III, *k*) was longer than the left (*g*)—54 mm. to 50 mm., but was somewhat narrower—17 mm. to 19 mm.; the outer margin of the left was uneven and undulating (see Fig. III, above *g*), and the entire body was as if curved around the shortened thalamus.[2] But the difference between the thalami was particularly remarkable; the length of the right thalamus amounted to 41 mm., that of the atrophied side to only 34 mm., while the latter presented an almost triangular form, in consequence of the marked projection of the inferior tubercle. The tænia between the corpus striatum and thalamus appeared swollen and thickened, in consequence of the thickening of the pia mater lying on it, and from the blood-vessel running along it several branches spread over the left corpus striatum, apparently the result of former chronic inflammation in these parts.[3] The glandula pinealis was large, and, in consequence of the unequal length of the thalami, lay rather obliquely; the commissura mollis between the thalami seemed to be dissolved. Of the corpora quadrigemina the left anterior was smaller than the right, the posterior horn of the left ventricle (*h i*) was much more dilated; and by the atrophy of the walls and the consequent stronger impression of the gyri, the cornu ammonis and the impressio digitata on the left side were better marked than on the right;[4] the entrance to the third inferior horn was much wider on the left side (*h*); the commissura maxima, as well as the fornix, was very soft, particularly on the left side.

At the base of the brain the difference between the two hemispheres was especially remarkable,[5] and was, in fact, so considerable, that the length of the right hemisphere (Fig. IV, *a*, *c*) exceeded that of the left (*d*, *f*) by 27 mm.[6] The left inferior lobe (*e*) in particular

[1] Fig. III, *c*. The difference in thickness between the convolutions of the healthy and affected side, which I have represented as faithfully as possible, is particularly well shown in these figures.

[2] This same curving of the corpus striatum, in a case of well-marked atrophy of the thalamus, is likewise shown, and is even still more strongly represented, by Cruveilhier, 'Anat. Path.,' livr. v, pl. v, fig. 3.

[3] Fig. III, at *g*.

[4] Fig. IV, between *h* and *m*; the impressio digitata in the direction of *i*.

[5] Fig. IV, *a b c*, *d e f*.

[6] See the table, in which the difference between the size of the parts on the right and left side of the whole body is accurately given in millimètres.

was very much atrophied, and its walls were very thin; the convolutions were here very small or had nearly disappeared, perhaps in consequence of the constant pressure of the fluid collected in the horn; the left crus cerebri was thinner, and the left corpus mammillare much smaller, than the right; the left half of the pons varolii and the left corpus pyramidale presented a much smaller circumference than on the right side;[1] the difference between the corpora olivaria was less.

In the cerebellum, on the contrary, the atrophy was on the opposite side, the right hemisphere (Fig. III, *f*) being in all its measurements much less than the left; consequently the so-called vermis or pyramis, posteriorly situated in the middle between the hemispheres of the cerebellum, was drawn obliquely to the right side (*h*).[2] No remarkable difference was manifest in the nerves, except that the optic nerve of the left side appeared to be somewhat slighter than the right, while the olfactory was shorter, and somewhat, although but little, more attenuated.

It is extremely remarkable that, while the medulla oblongata above the decussation is larger on the right side, the right corpus pyramidale agreeing with it in this respect, beneath the decussation, as appears from the transverse section of the spinal cord, the left or opposite side is thicker; so that below the decussation the atrophy of the left hemisphere has transferred itself to the right side of the spinal cord, as well as of the cerebellum; the atrophy of the right side of the cord was very evident at the level of from the fifth to the seventh cervical vertebra.[3]

In some measure corresponding to this I found the roots (but particularly the ganglia on a level with the brachial plexus), of the

[1] See Fig. III, and the table given at the end. In this figure I have drawn the medulla oblongata shortened, in order the better to show the difference in circumference between the two sides; it is placed in precisely the natural direction which it occupies during life, as I saw clearly from a cast and moulding of this brain prepared by Heer Schubard, Demonstrator of Anatomy, which fitted exactly into the skull. All the drawings with which I am acquainted, representing the base of the brain, err in the precise direction of the medulla oblongata with respect to the brain.

[2] See the particular measurement and difference in size of these several parts of the cerebellum in the table appended to this work.

[3] In order to spare the skeleton, which, as we shall see, likewise offers so many important points of comparison, the spinal canal was not further opened.

fifth, sixth, and seventh cervical nerves on the right side, much diminished in thickness and size; this difference was particularly striking in the fifth and sixth nerves.[1] So far as I have been able to ascertain, no writer seems to have directed his attention to this difference in thickness of the spinal cord and plexuses on the side opposite to the cerebral atrophy. The trunks of the nerves, too, in the neighbourhood of the ganglia were slighter on the right than on the left side,[2] which is of especial importance, since, as we shall hereafter see, the opposite condition seems to obtain in the further course of the nerves.

The state of the skull is not less important, the difference in the thickness of the two sides being very striking,[3] although externally scarcely any difference is observable between the right and left. In the first place, in the left half of the skull, both in the vault and on the base, all the so-called impressiones digitatæ and juga cerebralia are wanting, while on the right, on the contrary, they are very distinct. This is particularly evident above the orbital plate, which on the left side is convex and perfectly smooth and even, without any impression (Fig. I, *a*), while on the left it is less arched and is marked with very deep impressiones digitatæ (*b*). The difference in thickness of these plates is very remarkable; thus, while that of the right amounts at *b* to only one millimètre, the bone being very transparent, the thickness of the left in the neighbourhood of *a* is not less than five millimètres. The cribriform plate between the two is rather oblique, and the crista galli (*q*) inclines so much to the left side as completely to cover its anterior half, in consequence of which even the olfactory nerve is compressed and rendered much shorter and slighter. The processus clinoidei antici have an oblique direction; that on the left side extends more backwards and is much thicker (*k*);[4] the processus clinoidei postici are smaller, and at the sides a new osseous plate runs from either edge to the os petrosum (*p*).

But the principal difference exists in the great wings of the sphenoid bone, on which the end of the middle lobe of the brain rests; that on the right side forms (*d*) a tolerably deep cavity and groove beneath the clinoid process, in which, as usual, the inferior

[1] Fig. V, *a*, *b*, *c*, and *d*, *e*, *f*.

[2] For details see the table.

[3] See Fig. I.

[4] This oblique position of the clinoid processes quite agrees, though in an inverse direction, with Weber's drawing, where the atrophy took place on the right side. See 'Acta Nat. Curios,' Bonnæ, 1828, vol. xiv, tab. xii.

cerebral lobe is situated; but that on the left side forms an inclined plane (*c*), passing obliquely downwards from the lower edge of the clinoid process, and, in place of a cavity, presenting a slightly convex surface. This gives rise to such a considerable difference in the bone, that while the thickness of the wing on the right side, at the part marked *d*, amounts to only 2 mm., on the opposite side (*c*) it attains not less than 20 mm. This wing of the sphenoid bone is separated on the left side from the squamous portion of the temporal bone by a very deep, venous groove, behind which the wall of the temporal bone projects inward in a strongly convex form (*o*); in the situation of this letter the thickness of the temporal bone amounts to 12 mm., on the opposite or healthy side to 2·5 mm. In like manner, the inner surface of the os petrosum is very much thickened on the left side, presenting a convex appearance, without any indication of the arching of the semicircular canals (*f*); the distance from the inner and upper edge of the external auditory passage to the surface of the petrous bone amounts, on the left side at *f*, to 19 mm., on the right to only 13½ mm. In addition, there are internally on the skull, wherever the latter is thickened, numerous grooves for blood-vessels, both on the arch and on the base, which on the opposite or sound side are either wholly absent or are much smaller.[1]

The condition of the skull at the occiput, or cerebellar region, is particularly remarkable. Here everything is reversed; just as we saw that in this situation the left half of the cerebellum is the larger, the right being diminished. To this the skull corresponds; not only is the left cavity for the cerebellum (*n*) much more spacious and larger than the right (*m*), but here the thickness of the skull is reversed; thus, while on the left side the cranial wall at the situation *n* is, through a tolerably great extent, very slight and transparent, amounting to about 1½ mm. in thickness, on the right side it is thicker, measuring at the thinnest part, at *m*, about 2½ mm.; while in the neighbourhood of this point the thickness rapidly augments to 5 mm., affording a striking proof how much the form of the skull adapts itself to that of the brain. No remarkable difference is discoverable between the foramina for the nerves on either side; the round opening for the second branch of the fifth pair alone seems to be somewhat wider on the left, but the carotid canal is much narrower at its entrance

[1] Figs. I and II.

into and exit from the petrous bone on the left side, thus corresponding to the diminished size of the atrophied hemisphere of the brain, in the proportion of 4 mm. to 6 mm. diameter on the healthy side. On the contrary, the foramen spinosum for the arteria meningea media (*r*), which runs through the deep groove on the left at *o*, and supplies the thicker cerebral wall with nourishment, is much wider than on the right side. The sinus rectus inferior, again, is very wide, and repairs, in an oblique direction, to the foramen jugulare of the right side (*i*), which is much wider than on the left or atrophied side, the diameter of this opening being only 3½ mm. on the left, while on the right it amounts to 6·8 mm. It is remarkable that exactly along the sinuses, both the longitudinal and transverse, and particularly on the right side, where they are wider, a thin, white, osseous plate seems to have separated on the rest of the bone, which is yellow, the contrast in colour rendering the former very conspicuous.[1]

Although externally no great difference is visible between the right and left side of the skull, a difference is evident in some points; thus, for example, the malar process of the upper jaw, and from the zygoma beneath the orbit, is somewhat thicker on the left side, measuring exactly in the middle under the orbit, 21¼ mm. on the left, and 20 mm. on the right, side. The zygomatic cavity for the temporal muscle is likewise larger on the left side, its longest antero-posterior diameter amounting to 37 mm., while that of the right side is only 35 mm.; the height from the coronoid process to the inferior margin of the jaw amounts on the left side to 61, on the right to only 58 mm.; whence it would seem that the greater nutrition of the left side of the skull extended also to the temporal muscle and to the lower jaw, although in a less degree. Hence it appears that, along the whole course of the fifth pair of nerves of the left side, which must have been more irritated, a greater degree of growth, and a greater deposition of new matter, must have taken place, a point of which I shall hereafter treat.

The unilateral atrophy of the rest of the body, and particularly of the superior extremity of the right side, consequently opposite to that of the brain, was not less strongly marked. Thus, the length of the clavicle on the left or sound side amounted to 140 mm., on the right to only 129; the circumference of this bone, and therefore its proportionate thickness, was an inch; of the sternum, on the left,

[1] Fig. II *a*, *a*.

36 mm., on the right 29; the length of the left scapula from the superior to the inferior margin was 152 mm., of the right it was only 136; the breadth of this bone from the margin of the articular cavity across to the dorsal edge was, on the left 97 mm., on the right 88 mm.; the length of the left humerus was 319, of the right 233 mm.; the circumference at the insertion of the deltoid was, on the left 59, on the right 48 mm., while the entire length of the left arm from the top of the humerus to the point of the longest finger amounted to 713 mm., that of the right to only 669, showing a differcnee of not less than 44 mm. But it is remarkable that this difference in length and thickness was particularly conspicuous in the upper portion, as the clavicle, scapula, and humerus, and was somewhat less in the radius and ulna, while the length of the hands from the upper edge of the os naviculare to the point of the middle finger was equal in both extremities, namely, 179 mm.; the thickness, or rather the circumference, of the metaearpal bone of the middle finger was somewhat greater on the left or unaffeeted side, viz., 27 mm. against 22 mm. on the right side. The difference between the inferior extremities, although considerable, was less decided than between the arms; thus, the length of the leg from the upper point of the external trochanter of the femur to the inferior edge of the anele was, on the left 767, on the right 734 mm.; while, therefore, the difference in the length of the arms was 44 mm., that of the legs amounted to only 33 mm. Again, the feet, from the posterior margin of the heel to the point of the great toe, were of precisely the same length, just as was the case with the hands; even the thickness of the os metatarsi of the great toe in the middle was somewhat greater, namely, 43 mm. on the right to 41 mm. on the left side, but the length of this bone on the left side exceeded that of the corresponding bone on the right, in the proportion of 57 mm. to 54 mm.

In the ribs the difference between the right and left sides, although less strongly marked, was distinct enough, but in a descending order. Thus, the difference in length between the first ribs of the right and left sides amounted to 20 mm., between the seventh to 6 mm., and between the eleventh to 2 mm. A similar inequality was also perceptible in the pelvis; the height from the upper edge of the crest of the ilium to the tuberosity of the ischium was, on the left 210, on the right not more than 204 mm.; from the upper margin of the pubis to the tuberosity of the ischium it was, on the left 97, on the right 93 mm.;

from the antero-superior spine of the ilium to the posterior tuberosity adjoining the sacrum it measured, on the left 136, and on the right 132 mm.[1]

It is evident that the atrophy of the right side of the body is most closely connected with that of the right half of the spinal cord and of the left hemisphere of the brain. It is remarkable that, as I shall show, this atrophy is almost invariably more strongly marked in the upper than in the lower extremities. This coincides with the fact that in hemiplegia the leg is usually less affected and recovers earlier and more completely than the arm.

The intimate connexion of the cerebro-spinal system with the nerves is particularly well shown in the remarkable atrophy of the ganglia of the sensory nerves on the right side; for this reason I have represented as accurately as possible the ganglia of the fifth, sixth, and seventh cervical nerves in Fig. V, where the considerable difference in size at once strikes the eye. The different thickness and breadth of the ganglia and of the nerve-trunks, shortly after the two roots have united to form a trunk, have been determined with a micrometer, which readily shows a variation of one tenth of a millimètre on a scale appended to it. The results of the measurements will be found in the following table:

Ganglia of the Cervical Nerves.	Direction of the measurement.	Right.	Left.	Difference.	Trunk of the Nerves.	Direction of the measurement.	Right.	Left.	Difference.
5th Ganglion	breadth	3·8	5·1	1·3	5th Nerve ...	breadth	2·9	3·8	0·9
	thickness	2·2	3·9	1·7		thickness	1·3	2·2	0·9
6th Ganglion	breadth	4·1	5·1	1.	6th Nerve ...	breadth	2 6	3·4	0 8
	thickness	2·5	3·8	1·3		thickness	2·4	2·6	0·2
7th Ganglion	breadth	5·	5·6	0·6	7th Nerve ...	breadth	3·7	4·3	0·6
	thickness	3·1	4·6	1·5		thickness	3·3	3·7	0·4

[1] This rendered the pelvis somewhat oblique. Weber, too, gives a drawing of the pelvis in the case of atrophy of the brain observed by him. It is, however, not easy to deduce a definite inference from his drawing; the left side of the sacrum was broader, and accordingly the entire left half of the pelvis was more spacious, more like the female pelvis; the right portion was smaller, but at the same time the bones on the right side were thicker, the acetabulum was larger, the os ilium was more bulky and broader. The atrophy of the brain existed on the right side, so that probably the left half of the pelvis must have been the atrophied portion. (See 'Act. Nat. Curios.,' 1828,

It is very remarkable that in the lower and further removed trunks of the nerves this attenuation on the paralysed side did not appear to exist; on the contrary, some thickening seemed again to be present in these parts, at least according to accurate measurements of the fresh nervus medianus of the arm, taken with reference to this point by my esteemed friend, Professor Harting. Thus, the diameter of the nervus medianus at the bend of the elbow was as follows:

	On the left or sound side.	On the right or atrophied side.
Diameter of the nerve . .	3·6 mm.	3·7 mm.
Primitive tubes .	from 8 to 21·7 mmm.	8·2 to 22·4 mmm.[1]
Average diameter in fifteen measurements	15 mmm.	15·5 mmm.

In this part of the nerve, however, the motor fibres were still commingled with the sensory. But a branch of the nervus medianus from the middle finger, which consequently contained only sensory, and at the same time perhaps vegetative, filaments, furnished primitive tubes of the following thickness:

	On the left or sound side.	On the right or atrophied side.
Diameter of the primitive tubes . .	from 11·1 to 21·4 mmm.	from 11·6 to 21·8 mmm.
Average diameter in five measurements	15·8 mmm.	15·4 mmm.

Hence it would appear that among the sensory filaments on the sound side there were more thick primitive filaments than on the atrophied side, but the number of measurements is, perhaps, too small to justify the deduction of any definite inference.

By other writers, too, mention is made of a thickening of the nerve-trunks on the paralysed side, which is probably produced by degeneration of the neurilema, and perhaps by the deposition of fat between the nerve-tubes. These nerves were more yellow, of which Lallemand brings forward three different cases.[2] In one case the nerves were thickened and red.[3]

Tab. xiv.) I have not been able to find any other examples of obliquity of the pelvis in unilateral atrophy of the brain, in consequence of the attention of authors not having been directed to this point.

[1] [The reader will please observe that the letters mmm. signify the millionth part of the mètre, or the thousandth part of the millimètre. This unit is usually employed by writers on physical optics, and in designating microscopic measurements.—TRANSLATOR.]

[2] 'Recherches anatomico-pathologiques sur l'Encéphale,' Lett. 8, Nos. 27, 36, 43.

[3] Lallemand, l. c., No. 49; at page 358 the author promises to return to

The difference between the muscles of the two sides of the body was very considerable, as appears from the following measurements by Professor Harting:

	Sound or left side.	Atrophied or right side.
	Deltoid.	*Deltoid.*
Greatest breadth	98 mm.	82 mm.
Primitive bundles	412 mmm.	282 mmm.
	Average diameter of 23 bundles.	Average diameter of 22 bundles.
	Biceps.	*Biceps.*
Greatest breadth	24·5 mm.	20·5 mm.
Primitive bundles	347 mmm.	285 mmm.
	Average diameter of 27 bundles.	Average diameter of 23 bundles.
	Flexor carpi radialis.	*Flexor carpi radialis.*
Greatest breadth	23 mm.	13 mm.
Primitive bundles	502 mmm.	303 mmm.
	Average diameter of 15 bundles.	Average diameter of 14 bundles.

This, calculated for 100, gives a proportion of the atrophied to the healthy muscles of 83·7 for the deltoid and biceps; for the flexor carpi radialis, on the contrary, the proportion is only 56·6. The thickness of the primitive bundles seems not to be the same in the different muscles; its diminution gives a per-centage thickness on the atrophied side for the deltoid of 68·4, for the biceps of 82, and for the flexor radialis of 60·4; whence it appears that the emaciation and atrophy was by far greatest in the flexor radialis, although the diminution in the bones of the forearm was much less than in the humerus, scapula, and clavicle.

A comparison of the foregoing case with others of a similar nature to be found in the writings of some authors, particularly in works of more modern date, will show that such an atrophy of one half of the brain is a lesion of rather rare occurrence. Although Morgagni speaks of an obliquity of the skull,[1] I have been unable to find a dis-

this subject, and remarks upon the strange contrast between the thickening of the nerves on the paralysed side and the atrophy of the half of the medulla spinalis in some cases. But nowhere in Lallemand's letters have I been able to find any further mention of the subject, nor any case containing an allusion to atrophy of one half of the spinal cord. Durand-Fardel gives a remarkable instance of softening of the brain, particularly of the right side, in which the medulla spinalis, especially in the dorsal region, was extremely softened and atrophied; the arachnoid also was very much thickened, but the atrophy seems to have existed on both sides. In consequence of the loss of his notes, the history of the case is very imperfect. (See Durand-Fardel, 'Traité du ramolliss. du Cerveau,' 1843, obs. 83, pp. 297 et seq.)

[1] 'De causis et sed. morb.,' epist. i, art. 14, and epist. lxii, art. 15.

tinct case of atrophy of one half of the brain, such as I have detailed, either in his writings or in those of Bonnet, Voigtel, or Meckel.

Much diversity of opinion prevails among writers as to the nature and cause of this lesion. As the atrophy I have described is said to arise either shortly after, or even before, birth, some, as Cazauvielh, have regarded it, not as an atrophy, but as a congenital defect, on which account this author has assigned to it the name of *agénésie cérébrale*.[1] Lallemand has, in my opinion correctly, rejected this idea, as the atrophy seems evidently to be the result of previous inflammation in the central parts of the brain, at one time occurring before, at another after, birth.[2] We must, in fact, distinguish this lesion from congenital defects properly so called, among which latter, for example, the remarkable case communicated by Rudolphi is to be included, of a stillborn child, in whom the right hemisphere of the brain was diminished in size, but at the same time the first four and the sixth cerebral nerves were wholly absent.[3]

The principal writers in whose works I have met with examples of a similar unilateral atrophy of the brain are Gall,[4] Weber,[5] Cruveilhier,[6]

[1] "Agénésie cérébrale" in the 'Archives générales de Médecine,' tome iv; also translated by Nasse in 'Sammlung zur Kentniss der Gehirn- und Rückenmarkskrankheiten, Stuttgart,' 1840, 3 Heft, pp. 1 et seq.

[2] 'Recherches anatom.-pathol. sur l'Encéphale,' Lett. 8, pp. 239 et seq., 351 et seq., and in several other places. Lallemand himself brings forward the case of a pregnant woman, who got a violent blow on the abdomen, followed by vigorous movements of the child, which wholly ceased after the lapse of twelve hours. Four days subsequently the woman was delivered of a dead child, in whom the entire of the right hemisphere of the brain was degenerated to the condition of a red pulp, still containing some less degenerated parts. L. c., p. 222; see also p. 226, obs. 24 and 25. At a later period of life, too, atrophy of one hemisphere may arise in consequence of chronic inflammation, with phenomena of semi-paralysis. See Baillarger, "De la cause anatomique de quelques hemiplégies incomplètes," in 'Annales Medico-psycholog.' Paris, 1858, pp. 168 et seq. Baillarger found a difference in weight between the two hemispheres of from twenty to sixty-two grammes.

[3] 'Abhandlungen der Königl. Academie der Wissenschaften zu Berlin,' 1814, 1815, pp. 185 et seq.

[4] Gall, 'Sur les fonctions du Cerveau,' 8vo, tome ii, p. 252, and tome iii, p. 97. Also in his 'Anatomie et Physiol. du Système nerveux,' tome iii, p. 36, pl. li, lii, liii. In the text Gall speaks of the left hemisphere of the brain, which was atrophied in different places. But in all the plates, probably from neglect of transposition of the engraver, the right hemisphere is represented as atrophied. I have thought it right to keep to the text.

[5] 'Acta Nat. Curios.,' Bonn, 1828, pp. 111 et seq.

[6] 'Anat. Patholog.,' fasc. v, pl. iv, v; fasc. viii, pl. v. In the one case

Pinel fils,[1] Breschet,[2] Cazauvielh,[3] Andral,[4] Lallemand,[5] Smith,[6] Romberg,[7] and Saint-Yves, who communicates a case where the one hemisphere of the cerebrum amounted to only two thirds of the other, while the opposite hemisphere of the cerebellum was atrophied.[8] Rokitansky, too, in his work on pathological anatomy, speaks of this lesion, but without adducing any new case of it.[9]

It is much to be lamented that all these observations are more or less imperfect; almost all the writers confine themselves to the state of the brain; I have nowhere been able to find any accurate observation of the unilateral atrophy of the spinal cord.

If we add my case to those of other authors, with which we may also enumerate that of an idiot communicated to me by Professor Schneevogt, where the left hemisphere of the cerebrum, as well as that of the cerebellum of the same side, was atrophied, we have in

(pl. iv, fasc. v) Cruveilhier makes a mistake in the text, in speaking of atrophy of the right hemisphere; in the plates the left hemisphere is represented as atrophied; and also at page 4 of the same case, Cruveilhier says that the left hemisphere was atrophied. Several writers commit mistakes of this kind.

[1] "Recherches anat.-patholog. sur l'endurcissement du Système nerveux," in Magendie, 'Journal de Physiol.,' tome ii, p. 191.

[2] Breschet, in 'Journal de Physiol.,' par Magendie, tome iii, 1823, p. 241.

[3] "Agénésie cérébrale," in Nasse, Sammlung,' l. c., 3 Heft. This writer communicates not less than twelve cases, of which, however, only the first six can with any certainty be admitted as cases in point, since in the others the author infers the existence of the lesion only from the symptoms, no dissection having been made.

[4] 'Clinique Médicale,' tome v, obs. 1—4, pp. 618 et seq.

[5] Lallemand, 'Recherches anat.-path. sur l'Encéphale,' lett. 8, pp. 221 et seq. In this communication nearly all the cases previously observed are to be found, with the exception of that related by Weber. They are collated and compared by the author with his usual acuteness and accuracy.

[6] 'Dublin Journal,' May, 1842, p. 296.

[7] 'Lehrbuch der Nervenkrankheiten,' 1 B., 3 Abth., p. 820. Also in Henoch's dissertation, 'De Atrophia Cerebri,' Berol., 1842, pp. 24 et seq., in which there is a review, although rather a superficial one, of previous cases, and of the phenomena observed in them.

[8] 'Archives générales de Médecine,' Oct., 1834, and Jan., 1835. The case is that of a patient, who was subject to epilepsy, and had lapsed into a state of total imbecility; he died in his thirty-sixth year. Turner, too, describes cases of atrophy of one hemisphere of the cerebrum with atrophy of the opposite hemisphere of the cerebellum. See 'Annales Med.-psychol.,' 1856, p. 452.

[9] 'Path. anat.,' 1 B., pp. 775 et seq. I am, however, aware that in the Museum at Vienna such a case is preserved, with atrophy of one hemisphere of the cerebrum; the cerebellum was not examined.

twenty-nine cases of unilateral atrophy seventeen instances where the lesion existed on the left side of the brain. Almost always the entire hemisphere of the cerebrum was more or less atrophied; in some cases the anterior lobe was particularly wasted,[1] in others the middle,[2] or the posterior,[3] was more affected; in a few cases no softening, but rather hardening, coexisted with the atrophy.[4] Simultaneous atrophy of one hemisphere of the cerebellum appears to be very rare; among these twenty-nine cases it occurred only seven times—five times on the opposite, and twice on the same side. Thiaudière, too, once found the hemisphere of the cerebellum softened on the same side.[5]

It is remarkable that the coexistent atrophy of the one hemisphere of the cerebellum, whether situated on the opposite or on the same side, appears to have had no influence on the paralytic symptoms. It is usually supposed that lesions of the cerebellum almost always produce paralytic symptoms on the opposite side,[6] according to Burdach, as 12 : 2.[7] In Weber's case, where the hemisphere of the cerebellum was much more decidedly atrophied than the right hemisphere of the cerebrum, there was no paralysis properly so called; in

[1] Lallemand, 'Recherches,' l. c., obs. 27, 34.

[2] Lallemand, l. c., obs. 33.

[3] Lallemand, l. c., obs. 36.

[4] Lallemand, l. c., obs. 50, 61.

[5] Lallemand, l. c., obs. 35; also in Magendie, 'Journal de Physiol., tome ix, p. 53, communicated by Piorry, but where no mention is made of this softening of the right hemisphere of the cerebellum. Gall, in his case, does not speak of the cerebellum. But if we look with attention at pl. liii of his great work ('Anatom. et Phys. du Syst. nerveux'), we shall see that the opposite side of the cerebellar cavity of the cranium is diminished in size; so that it is extremely probable that in this instance, too, atrophy of the cerebellum existed on the side opposite to that of the cerebrum. Clendinning also communicates a case of a child, who died at eighteen months, of pneumonia with convulsions. Scrofulous affections were found in different places, as in the middle lobe of the cerebrum on the left side, and also on the dura mater and in the lungs, together with a remarkable formation of the cerebellum, of which the right hemisphere, consequently again that of the opposite side, was from one half to two thirds smaller than the left. See Schmidt, 'Jahrbücher,' Supplem., Band i, p. 299. Also in the 'London Medical Gazette,' vol. xvi, May and June 6, 1835. Alphonse de Saint Germain describes a remarkable case of very great atrophy of one hemisphere of the brain, without atrophy of either side of the cerebellum, or of the medulla oblongata. 'Annales Medico-Psychol.,' 1856, p. 613.

[6] Longet, 'Traité de Physiologie,' tome ii, p. 260.

[7] Burdach, 'Ueber das Gehirn,' B. iii, p. 370, § 802.

Cruveilhier's case, where well-marked atrophy of the left side of the cerebellum was found, with atrophy of the thalamus and particularly of the middle lobe of the cerebrum on the same side, no paralysis existed; at least the child brought both hands to its mouth.[1] In my case, too, the paralysis was evidently the result of the opposite atrophy of the cerebrum; the atrophy of the one hemisphere of the cerebellum seemed in this instance to have given rise to no symptoms.[2] Generally speaking, I suspect that the phenomena of disturbed movement in vivisections after lesions of the cerebellum are to be explained as the result of irritation conveyed through the fibres of the pons varolii from the cerebellum to the corpora pyramidalia, whereby we can further easily account for the fact that these lesions produce their effects on the opposite side, whither they are transferred through the inferior decussation of the corpora pyramidalia. The experiments of Flourens and Hertwich, therefore, appear to me properly to prove nothing for the direct influence of the cerebellum on motion, much less on the so-called co-ordination of movement; as we see that in a frog, where the whole head with the cerebellum is cut away, irritation of the hind foot is capable of exciting by reflexion jumps and perfectly co-ordinate movements, the cause of which is, therefore, to be sought, not in the cerebellum, but in the spinal cord.

In twenty-one of these twenty-eight cases, the condition of the corpora striata and thalami is stated; in seventeen, both these parts were diminished in size on the atrophied side; in three, the thalamus of the atrophied hemisphere was smaller;[3] in one case only the corpus striatum appeared to be affected, while the thalamus was healthy;[4] in three instances it is distinctly stated that the corpora striata and thalami of both sides were equally large and sound,[5] although here, too, paralysis existed on the one side.

Much more rarely do we find any change in the pons varolii described; in the greater majority of cases writers seem not to have

[1] Cruveilhier, 'Anat. Path.,' livr. v, obs. 3, pl. v.

[2] Romberg, too, remarks the same, 'Lehrbuch,' l. c., p. 819.

[3] Cazauvielh, in Nasse, l. c., 4 Beob., p. 15. See also Lallemand, l. c., obs. 28, and Cruveilhier, 'Anat. Path.,' livr. v, obs. 3, pl. v. Also in Lallemand, obs. 33, and Lallemand, obs. 37. Gall, 'Sur le Syst. nerveux,' tome iii, p. 36.

[4] Lallemand, l. c., obs. 40.

[5] Cazauvielh, in Nasse, l. c., 2 Beob. and 6 Beob., and Lallemand, l. c., obs. 41.

attended to it, or to have only superficially compared its form on both sides. Four times it is stated that this part was in the normal condition.[1] In Weber's and Romberg's cases,[2] as well as in mine, the pons varolii was evidently thinner and atrophied on one side; in Romberg's case and in mine, on the atrophied side of the brain, which accords with the greater thinness of the crus cerebri. In Weber's case it is, singularly enough, stated that, while the right hemisphere of the brain was smaller, the left crus and the left side of the pons were atrophied.[3] In a few instances mention is also made of the corpus mamillare; in Cruveilhier's case,[4] in Romberg's,[5] and in mine, it was smaller on the atrophied side; in Weber's, on the contrary, it was larger.[6]

The several writers seem to have paid equally little attention to the state of the corpora pyramidalia, the corpora olivaria, and the medulla; in general, no mention is made of these parts, while it is tolerably evident, from the description, that sufficient attention has not been given to them. In a case by Thiaudière it is said that both bodies were atrophied on the affected side.[7] In Romberg's case the corpus olivare was thicker on the atrophied than on the opposite side.[8] Perhaps this is true also of Weber's case.[9] In my case the

[1] Lallemand, l. c., obs. 27, 35, 44, 46.

[2] Romberg, 'Lehrbuch,' 3 Beob., l. c., p. 822.

[3] Weber, in the 'Acta' of Bonn, l. c., pp. 114 and 117. On the whole, such confusion prevails in Weber's description between the state of the right and left side that it is difficult to draw a conclusion from it. Thus, for example, at page 117 it is said that the corpora pyramidale, olivare, and restiforme of the right side are larger, while, at the following page, in the table of separate measurements, the left corpus olivare occurs as the larger. The coexistence of atrophy of the left crus cerebri (l. c., page 114) with atrophy of the right hemisphere of the brain seems very strange to me; in the drawing (pl. xi, fig. 1) the right hemisphere of the brain appears rather larger; in the plates of the skull (pl. xii and xiii) it is seen, on the contrary, that the left side was larger.

In a case by Carswell atrophy of one side of the pons varolii also occurs, together with atrophy of the crus cerebri, corpus olivare, and corpus pyramidale (see 'Path. anat.,' fasc. x; 'Atrophy,' pl. iv, fig. 2). But this case appears not to be one of the so-called congenital atrophy, for which reason I have not enumerated it with the foregoing.

[4] 'Anat. Path.,' livr. viii, pl. v, fig. 2.

[5] Romberg, l. c., p. 822.

[6] Weber, l. c., tab. xi, fig. 1 *e*.

[7] Lallemand, l. c., obs. 37.

[8] Romberg, l. c., p. 822.

[9] See note [3] above.

corpus olivare on the atrophied side appeared to be somewhat smaller, although not so much wasted as the corpus pyramidale. It is remarkable that Cruveilhier, in a case of atrophy of the two hemispheres, found the corpora pyramidalia likewise on both sides atrophied, while the corpora olivaria, on the contrary, were very highly developed. Hence it would seem to follow, that the corpora olivaria are less directly related to the cerebrum than the corpora pyramidalia; this point must, however, be decided by future investigations.[1]

Of the medulla oblongata itself, and its greater tenuity and atrophy beneath the decussation on the opposite side, not one of the writers speaks, and it would seem that no one has observed it. Nevertheless, it appears, from Weber's drawing, that this state of things existed in his case, although he makes no mention of it, notwithstanding that the difference is strikingly represented.[2]

I believe that if an accurate investigation had been instituted in every case, unilateral atrophy of the spinal cord on the paralysed side would, in most instances, have been discovered.

From this brief retrospect we see that atrophy of the corpora striata and thalami is by far the most frequent attendant on wasting of the cerebrum. It appears to me to be probable that, at least in the majority of instances, the lesion in these parts commenced in consequence of inflammation; thus, the largest blood-vessels from the base of the brain here pass inwards, and the corpora striata, in particular, belong to the most vascular parts of the brain, while the white medullary matter, which constitutes the mass of the cerebrum (centrum semiovale Vieussensii), derives its vessels chiefly from those of the corpora striata, and scarcely at all from the cortical substance; satisfactory reasons why, if inflammation arises in these central parts, it should very easily extend to the surrounding medullary matter and produce softening. But as it is well known that the pia mater covering the brain externally very easily conveys its affections to its prolongations investing the cerebral cavities, as is so commonly seen in old cases of insanity and dementia, the inflammation at this earlier period of life may in some cases have spread from without to the

[1] 'Anat. Path.,' livr. v. "Idiotie," obs. 2, p. 5. The greater size of the corpus olivare may sometimes be apparent, as, where the corpora pyramidalia are much atrophied, the corpora olivaria must be more prominent. I have already spoken of Carswell's case, where the corpus olivare was really smaller on the atrophied side.

[2] L. c., tab. xi, fig. 1, r.

central parts; usually, however, the inflammation is in this case not limited to one hemisphere, but extends over both.[1]

With respect to the symptoms produced by this unilateral atrophy of the brain, they manifest themselves partly in the more or less defective exercise of the mental powers, and partly extend their influence, as I have shown at length in the above case, over the rest of the body. That in atrophy of one half of the cerebrum the psychical powers should be blunted or paralysed, might perhaps be assumed as generally true (and, in fact, such atrophy is most usually met with in idiots); still it is far from being universally the case, for although in some instances mention is made of rather blunted mental powers, examples also occur where, with atrophy of one hemisphere, the intellectual faculties appeared to be in their normal condition.

Andral reports the case of a man, who died in his twenty-eighth year, and who, when a child of three years, had, after a fall, continued paralysed on one side; the right hemisphere of the brain was so completely atrophied that the pia mater formed a cyst, in which not a trace of cerebral matter remained. This membrane constituted the upper wall of a large cavity, the floor of which alone was formed by the thalamus, the corpus striatum, and all the parts found on a level with these two bodies: so that, observes Andral, nothing remained

[1] Although I have to regret that my endeavours to obtain further information as to the earlier condition of the patient and the nature of the symptoms in my case have been unsuccessful, we may find the phenomena which usually occur in these cases more fully developed by Lallemand. See 'Recherches sur l'Encéph.,' lettre viii, pp. 298 et seq.

As this lesion commences so early as, and perhaps sometimes even before, birth, there has rarely been opportunity of accurately observing its earlier phenomena; most of the latter were of a spasmodic nature, and were accompanied with stiffness and rigidity of the muscles, which passed into a slowly progressive paralysis, interrupted, however, by intermissions of shorter or longer duration, which Lallemand regards as a species of relapse (l. c., pp. 301 and 303), produced by repeated inflammatory congestions. Resting chiefly on the results of post-mortem examinations, I quite agree with Lallemand, that an inflammation of a chronic nature, which in the brain (not being a very irritable part) often manifests no very striking phenomena, is at the bottom of these attacks. The course of these phenomena agrees closely with that of those usually met with in softening of the brain, also at a later period of life, especially in the cases where the corpus striatum is at the same time affected; we find, among others, some examples of this in the work of Durand-Fardel, 'Traité du ramollissement du Cerveau,' Paris, 1843, obs. 81—83, and 86. Here, too, when softening lasts long, absorption and atrophy supervene.

of cerebral matter above the ventricles except what was in front of the corpus striatum and formed the inferior wall of the cavity. And yet he testifies that this man had received a good education, had a good memory, a good address, and exhibited as much intelligence as most men.[1]

The same is stated by Thiaudière of his patient, whose understanding was of ordinary calibre, and whose mental powers remained clear to the period of his death (he died of pulmonary phthisis). Nothing remained of the right hemisphere of the brain but the atrophied corpus striatum and the thalamus.[2]

In the case communicated and delineated by Cruveilhier, in which the left hemisphere was so remarkably impaired and atrophied, the intellectual powers were unaffected.[3]

Weber testifies the same of his patient.[4] But as in this instance

[1] "La voûte du crâne ayant été enlevée, on trouva les méninges du côté droit transparentes et fluctuantes dans presque toute leur étendue. On les incisa, et il en jaillit une grande quantité de sérosité claire et liquide, comme de l'eau de roche. Entre ces méninges et les ventricules, il n'existait pas la moindre trace de substance nerveuse; ces membranes constituaient la paroi supérieure d'une vaste cavité, dont la paroi inférieure était formée par la couche optique, le corps strié et tout ce qui se trouve au niveau de ces deux corps. Il ne restait de la masse nerveuse située au-dessus des ventricules, que celle qui en avant du corps strié en forme la paroi inférieure.

"Cet individu avait reçu de l'éducation et en avait profité; il avait une bonne mémoire; sa parole était libre et facile; son intelligence était celle du commun des hommes."—Andral, 'Clinique médicale,' tome v, obs. 1, p. 618. See also Lallemand, l. c., obs. 26.

[2] Magendie, 'Journal de Physiol.,' tome ix, pp. 53 et seq. "Les facultés intellectuelles se conservèrent jusqu'au dernier moment; Vauquerse mourut en s'entretenant de ses souffrances avec Thiaudière, élève externe. Pendant le séjour du malade à l'infirmerie, l'intelligence était ce qu'on la trouve chez le commun des hommes."—Copied, with some abridgment, by Lallemand, l. c., obs. 37.

[3] "A la manière dont il répond aux questions qui lui sont faites, et dont lui-même exprime ses désirs, ses facultés intellectuelles paraissent entières. Il jouit de l'usage de tous ses sens, et d'après les renseignemens récueillis à la Chapelle près Paris, où il demeurait, Augé avait une intelligence ordinaire."—Cruveilhier, 'Anat. Path.,' livr. viii, pl. v. In Saint Germain's case, too, the woman, who died in her forty-first year, had retained her intellectual powers; the anterior lobe of the atrophied hemisphere appeared, however, to be less affected. 'Annales Méd.-psychol., 1856, p. 615.

[4] "Se fortem atque animosum præstitit, ut nullum vestigium proderet abnormis animi actionis, neque ullam malæ cerebri conformationis inveniendæ suspicionem moveret."

one hemisphere of the brain appeared to be less involved, I will attach no importance to it in this respect. Boulanger finally gives a case of a child, who, in its second year, was attacked with paralysis of the right side and contraction of the limbs, and which, until its death, in its fourth year, exhibited unimpaired mental powers,[1] and, nevertheless, the convolutions of the left anterior lobe of the brain were atrophied, hardened, and yellow.

It is, in fact, a very important phenomenon, that the loss of one half of the brain may be combined with the perfect use of the intellectual faculties. Lallemand, in his otherwise acute observations on these cases, endeavours to throw doubt upon their accuracy; thus, he thinks it may be assumed that the loss of mental power has in these instances kept pace with the degree of paralysis in the limbs; but that the latter is more evident because we compare the remaining action of the sound half of the brain with the morbid state of the affected side of the body, so that in these cases the deficiency of intellectual power is less observed than the paralysis.[2] But, in my opinion, he is not wholly impartial on this subject, as in the subsequent explanation of the particular cases where the intellect appeared to be unimpaired he either endeavoured to throw doubt upon the case, and consequently upon the accuracy of the writer, as of Thiaudière; or, as in Andral's observation, which he admits to be most accurate, seeks to explain it by supposing that the anterior part of the hemispheres of the brain was uninjured;[3] which, it appears to me, is plainly refuted by Andral's own words, as quoted above. It is equally incorrect to compare the perfection of the intellectual faculties with the greater or less degree of paralysis which was present, by which mode Lallemand endeavours to save himself.[4]

[1] "Les fonctions intellectuelles restèrent saines, il parlait bien, aimait à jouer, à causer, et parvint ainsi jusqu'à l'âge de quatre ans, époque à laquelle il mourut d'une péripneumonie."—Lallemand, l. c., obs. 45. To this I may add a case, communicated by Gall, of a clergyman, who suffered for a very long time from an erysipelatous affection of the forehead, and whose entire left side was so debilitated that in walking he was obliged to lean upon a stick; at length he died some hours after an attack of apoplexy. Three days before his death he had preached and catechised; after his death Gall found the half (he does not, however, say which half) of the right hemisphere of the cerebrum completely softened, and changed into a yellow granular substance. 'Sur les fonctions du Cerveau,' tome ii, p. 247.

[2] Lallemand, l. c., pp. 328 et seq.

[3] L. c., p. 339.

[4] L. c., p. 339.

We should compare it, on the contrary, with the degree of loss which the one half of the brain had undergone, and not with the greater or less degree of paralysis which may exist without impairment of the mental powers; as the cause of the paralysis is situated in other parts, which cannot be said to be the direct instruments of our higher faculties. Thus, in Andral's case, there was complete paralysis of the arm, with stiffness of the leg, which did not once occur in all the cases where idiocy existed, as in that described by me. Therefore Lallemand's assertion, that the impairment of the mental powers in these cases is in proportion to the degree of paralysis, is incorrect.

Everything, in my opinion, depends upon the more or less healthy state of one hemisphere of the brain. If, as from the nature of the case seldom occurs, the inflammation and affection of the pia mater has not extended to this hemisphere, if the gray matter under the cerebral convolutions has here continued perfectly sound, there is no reason why this remaining hemisphere should not be able to act without impediment in the exercise of those functions which are necessary to our mental powers, just as one eye sees as sharply though the other be lost. But where the gray matter is injured in both hemispheres, particularly anteriorly, disturbance of the intellectual faculties will be inevitable. I myself possess in my collection one hemisphere of the brain of a man, aged seventy-two, which is extensively destroyed by softening superiorly and anteriorly, in consequence of chronic inflammation proceeding from the corpus striatum; and, in this case, although there was complete paralysis of the entire of one side of the body, the intellectual faculties were quite unimpaired up to the moment of death; the patient even assured me, shortly before his decease, that he never had had any feeling of headache. But in this instance the meningitis had not extended over the other hemisphere, and had thus excited no disturbance, while the part that was destroyed could not act, and was therefore incapable of giving rise to any confusion. In fact, we cannot apply the measurement of rule and compass to the exercise of the mental powers, and that parts of the brain may be lost without impairment of these powers is a familiar fact.[1]

[1] A remarkable case, where there was probably atrophy of one half of the brain, is communicated by Wigan in Forbes Winslow's 'Journal of Psychological Medicine.' (See Damerow's 'Allgem. Zeitschrift für Psychiatrie,' Berlin, 1851, 8 B., 2 H., p. 279.) A boy, aged fifteen, had an inequality of the skull, as if the

It is very remarkable that, in cases of such considerable destruction of the brain, the paralysis on the opposite side is usually incomplete, being often combined with contraction of the flexors, particularly of the fingers or the forearm. In almost all cases the arm was more affected than the leg;[1] yet perfect paralysis, and then only of the arm, appears to be of rare occurrence.[2] In all the paralysis was combined with a greater or less degree of atrophy or emaciation, not only of the muscles, but also, as in my case, of the bones themselves, so that one or both extremities of one side appeared to be much shorter than those of the healthy side. Sometimes this was rendered less striking, in consequence of an abundant production of fat in the affected part, which was, however, attended with atrophy of the bones or muscles.[3]

Sensation was less blunted, and usually appeared to be but little interfered with,[4] even where the muscles had lost their power of action.[5] Hence it would seem to follow, as can be proved by many other arguments, that the perception of feeling is situated, not in the brain, but in the medulla oblongata. But the atrophy occurring in a greater or less degree in one or both limbs is of great importance; Lallemand and several other writers are inclined to attribute this solely to want of exercise and motion;[6] Lallemand even states that

left half of the brain was cut off from above towards the ear, and was covered with a flat bone, so that the size of the left hemisphere could certainly not amount to more than one third of that of the right; at the same time he had a tottering gait, without being paralysed. This boy, who at first appeared to be quite idiotic and incapable of learning or understanding anything, with a stupid appearance, had, under constant instruction, three years later grown into a strong lad, and his intellectual faculties were fully developed, while his brain was increasing in size, the left hemisphere, however, always continuing about one third less than the right.

[1] Cazauvielh, in Nasse 'Sammlung,' l. c., p. 33. Lallemand, l. c., p. 318. Romberg, l. c., p. 843.

[2] Lallemand, l. c., obs. 26, 36, 38, 41.

[3] Cazauvielh, in Nasse 'Sammlung, l. c., p. 33. Cazauvielh's fourth case, of paralysis and atrophy of the same side as the atrophy of the cerebrum, at first sight appears very strange; as, however, there was a diseased part also in the right hemisphere, the above is easily explained, particularly as the affection of the left hemisphere was not very great. In this case we have also an exceptional instance of a greater degree of paralysis and atrophy of the leg than of the arm (l. c., p. 16).

[4] Romberg, l. c., p. 843.

[5] Lallemand, l. c., p. 320.

[6] Lallemand, l. c., p. 326.

he has seen atrophied muscles become thicker and regain their former circumference under the repeated application of galvanism.[1] Brown-Séquard also observed the same.[2] Others attribute the atrophy to a direct want of nervous influence. The importance of the subject requires that we should examine this point a little more accurately, particularly as there is still so much difference of opinion among physiologists as to the question of the more or less direct influence of the nerves on nutrition.

If, in the first place, we compare the several cases on record, we shall find that it is not true that limbs which are most paralysed are always the most atrophied. In the first case related by Cazauvielh the arm was thinner, but almost as long as the other, and its movements were very difficult; the leg, on the contrary, was shorter, obliging the woman to limp; still she could walk, proving that the leg was less paralysed than the arm.[3] In his fifth case the arm was rigid and immoveable, although as thick as the other (perhaps on account of increased development of fat); at the same time the leg was not only thinner, but was also shorter; she walked with the aid of a crutch, while she was wholly unable to use the arm.[4]

It is, however, much to be regretted that none of the authors have taken the trouble to measure the difference in thickness and length of the extremities, on which account I shall not attach much importance to Cazauvielh's two former inconsistent cases, but shall prefer to endeavour to draw some conclusions from my own. That the emaciation of the muscles is directly proportionate to the greater or less degree of paralysis, is beyond doubt; but the atrophy of the bones appears not to be so completely dependent on the latter. If, for example, we compare the muscles and bones of the arm in my case, it will be seen that the muscles of the humerus were much less atrophied than those of the forearm, in the ratio of 83·7 to 56·6.[5]

[1] L. c., p. 328.

[2] 'Annales Médico-psychologiques de Baillarger,' Paris, Juillet, 1850, pp. 453 et seq.

[3] L. c., p. 7. Lallemand, obs. 44.

[4] L. c., p. 19. Since the publication of this essay I have six or seven times observed in living subjects this lesion with shortening and atrophy, especially of the arm, opposite to the atrophy of the hemisphere. In the majority of cases a greater or less degree of idiocy existed; perfectly unimpaired intellectual powers I never witnessed with this lesion.

[5] See above, p. 95. The other muscles, too, of the forearm, as well as the flexor radialis, were greatly atrophied.

But in the bones of the arm we find a completely different state of things. If we look to the table appended to this essay, containing the measurements of the several parts, we shall see that the atrophy is most strongly marked in the clavicle and scapula, and steadily diminishes as we pass downwards towards the hand. If we reduce the difference in length and thickness of these bones to decimals, in order to enable us to compare them accurately, we shall obtain the following results:

Length of the clavicle	= 129 : 140 = 0·92 ;	thickness	= 28 : 36 = 0·777
,, scapula	= 136 : 152 = 0·895 ;	,,	=
,, humerus	= 293 : 319 = 0·918 ;	,,	= 48 : 59 = 0·81
,, radius	= 197 : 215 = 0·916 ;	,,	= 34 : 40 = 0·85
,, ulna	= 220 : 235 = 0·936 ;	,,	= 41 : 46 = 0·89

Hence we see that the difference between the several bones is least as regards their length, that the scapula has diminished most in length, and that the ulna presents the least difference, while the length of the hands is exactly equal. As to thickness, the difference is greatest in the clavicle, and presents a decreasing order in the humerus, radius, and ulna; that is, the difference is most marked in the bones, the muscles of which, the deltoid and biceps, are the least atrophied; therefore the bones of the upper arm are most atrophied, while in the forearm and fingers, where the atrophy of the muscles is greatest, the bones present the slightest difference.

But if we now reflect upon the action of the muscles, it will at once strike us that this cannot exercise as much influence upon the clavicle as upon the bones of the forearm and hand, which are much more moved by the muscles, while the motion of which the clavicle is capable must always be slight. If, therefore, want of motion and rest were the cause of the atrophy of the bones, the latter should be much more strongly marked in the fingers and forearm than in the clavicle, as this bone is never so much moved, and therefore, from the nature of the case, the difference of motion must be much less in it than in the hand and fingers, the more so as there was no complete paralysis of the upper arm, as satisfactorily appears from the less degree of atrophy present in the deltoid.

It therefore seems to me that, in addition to the effects of rest and want of motion, we must also take into account the consequences of diminished direct influence of the nerves on nutrition: and this appears to me the more probable when I reflect upon the considerable degree of atrophy which, as I have above stated, was found in the

so-called nerve-plexuses, or ganglia of the fifth, sixth, and seventh cervical nerves (see Fig. V). From the table already alluded to (p. 141), as well as from the drawing, it would appear that the ganglion of the fifth cervical nerve had undergone the greatest degree of diminution; the trunk also of this nerve, close to the ganglion, was most atrophied; the difference in the two nerves was somewhat less in the sixth pair, and least of all in the seventh; in the motor roots this difference was not so evident, at least it was in proportion infinitely less, so much so, in fact, that I could not estimate it by measurement. If we now attend to the course and distribution of the nerves, which unite (after decussating in the brachial plexus) to form different nerve-trunks, it will appear, from Kronenberg's investigations, that the scapula and certainly also the clavicle, receive their supply from the fifth cervical nerve, and that the other parts of the arm, at least the muscles, receive nerve-filaments, which arise from all the subsequent nerve-roots.[1] As the axillary nerve, in particular, also arises from the fifth and sixth cervical nerves, it would appear that the fifth and sixth nerves give their principal branches to the scapula, clavicle, and humerus, while the parts of the forearm receive their branches from the lower cervical nerves, the ganglia of which were less atrophied.

But it is well known that the ganglia are situated only in the sensitive nerves, and that the motor nerves take no part in them, and, again, that the sensitive nerves exercise an especial and important influence on nutrition, and much more so than the motor nerves. The varying degree of atrophy of these ganglia is therefore, in my case also, in direct connexion with the varying degree of atrophy of the bones of the arm, into which the muscular nerves do not pass; and hence it appears to me that the inference is quite admissible, that the greater atrophy of the fifth and sixth ganglia was the immediate cause of the greater atrophy of the scapula, clavicle, and humerus, which could not be derived from the atrophy and inactivity of the muscles.

The use of these ganglia in the sensitive nerves is as yet unascertained. But it appears to me indubitable that they are connected with the influence of the nerves on vegetative life; this is shown both by Magendie's well-known experiment of dividing the fifth nerve in the cranium, and by different cases of affections of the Gasserian gan-

[1] H. Kronenberg, 'Plexuum nervorum Structura,' Berol., 1836, pp. 45, 47, tab. i, fig. 2; tab. ii, fig. 2; tab. iii and v.

glion of this nerve:[1] so that if the first branch of the fifth pair is cut through, atrophy of the eye ensues, but this does not take place if the whole trunk of the nerve is cut through above the ganglion, leaving the ganglion in connexion with the first branch of the fifth pair.

This vegetative influence of the sensitive nerves extends even over the muscles, and when the sensitive nerves, united with the motor, pass through the muscles, they appear to give off vegetative branches to the latter. This is established chiefly by the important experiments of Longet, who found that, after the division of the branches of the fifth pair, the muscles of the face atrophied and became disobedient to stimuli more rapidly than after division of the facial nerve. Indeed, after division of this last nerve irritation of the divided ends excited no contraction in the muscles after the fourth day; but on the direct application of galvanism to the muscles themselves, the latter maintained their contractility for at least three months, and exhibited scarcely a trace of atrophy; after dividing the hypoglossus

[1] Magendie et Desmoulins, 'Anatomie des Animaux à vertèbres,' Paris, 1825, p. 710. 'Journal de Physiol.,' tome iv, p. 176. See also among several cases an example of affection of the Gasserian ganglion, with atrophy of the eye, in Serres' 'Anatomie comparée du Cerveau,' tome ii, pp. 67 et seq. By the experiment of Dr. Snellen, in his dissertation 'On the Influence of the Nerves on Inflammation,' Utrecht, 1857, the conclusion drawn from Magendie's experiment is rendered doubtful. According to Snellen, the cause of the inflammation of the eye after the division of the fifth nerve is the result of mechanical injury of the eye, as the animal does not feel that he hits it. But if the eyelids be sewn together, and the ear, which has remained sensitive, be fixed in front of the eye, the animal no longer hits the latter, and in this case the insensible eye does not become inflamed. In one instance I saw the eye clear after the lapse of ten days. Nevertheless, I doubt whether atrophy would not have supervened after a longer period.

It is, however, absolutely certain, that on division of the first branch of the trigeminus, vascular paralysis and dilatation of the vessels in the eye set in, as Schiff has subsequently pointed out. 'Lehrbuch der Physiologie,' 1860, 1 Th., pp. 382 et seq. But the influence of the ganglia on nutrition and the development of inflammation is placed beyond all doubt by the experiments of Dr. Samuel. 'Schmidt's Jahrbücher,' 1859, No. 10, p. 102. This writer irritated the Gasserian ganglion by the introduction of two needles, through which he passed a galvanic current, whereupon the conjunctiva immediately became red, the secretion of tears was increased, the sensibility of the eye was exalted, and violent ophthalmia set in within twenty-four hours, and passed into superficial ulceration of the cornea.

It would, however, appear that, as Schiff has pointed out, it makes no difference whether the division take place before or behind the ganglion, as Magendie asserted. 'Physiol.,' p. 385.

or recurrent, the author obtained similar results.[1] But after the branches of the fifth pair, which unite with the facial (the suborbital, the buccal, and the auriculo-temporal), were cut through in a dog, without injuring the facial, the muscles were at the end of six weeks discoloured, and, although still irritable, were much less sensitive than on the sound side; while after division of the facial alone the irritability was still unchanged at the end of twelve weeks.[2] From this experiment we may therefore infer that the sensitive nerves exercise much more influence on nutrition than the motor nerves do, and that consequently the rest which follows the division of the muscular nerves is at least not the sole cause of the atrophy of the bones, although I will not deny the influence of the muscular nerves on the paralysis and atrophy of muscles, apparent in many cases of the former affection. [In the 'Dublin Quarterly Journal' for August, 1852, vol. xiv, p. 248, I have myself made some observations which bear some resemblance to the above. They must have been penned about the same time, the date of Professor Schroeder van der Kolk's essay being also 1852.—TRANSLATOR.]

If we reflect upon the mode of action of the sensitive nerves, we shall find that it is centripetal towards the brain; and therefore the special sensitive filaments, which in my case (as in most instances of unilateral atrophy) had not lost their sensation, cannot influence vegetative life, which would require a centrifugal action. Now, the coexistence of the latter with centripetal action would be absolutely impossible, since a double action in one and the same nerve-filament, simultaneously extending centrally and peripherically, is an absurdity unsupported by a single fact. But as it is equally indisputable that the sensitive nerves do exercise a peripheric action, the latter must be attributed to the numerous sympathetic filaments, present in the sensitive nerves, which confer upon these nerves a power of peripheric action they would otherwise not possess. It is well known that Bidder and Volkmann have demonstrated these filaments, and indeed in much greater number in the sensitive than in the motor nerves, and they have also shown that they are most numerous where any more decidedly vegetative function—for example, a secretion—is fulfilled.[3] Although from several quarters arguments

[1] Longet, 'Physiol.,' tome ii, pp. 51 et seq.
[2] Longet, l. c., p. 59.
[3] F. H. Bidder und Volkmann, 'Die Selbständigkeit des Sympath. Nervensystem,' Leipsic, 1842, pp. 62 et seq.

have been brought forward against many points in the essay of these writers and the characteristic difference in thickness between spinal and vegetative nerves, as well as against the origin of the sympathetic in general, as suggested by Bidder and Volkmann, I am not aware that the fact based upon their investigations, that many more sympathetic filaments are contained in the sensitive than in the motor nerves, has as yet been fundamentally refuted, or even rendered doubtful. On the contrary, this appears to me to be in every respect established by the reasons quoted above, and I therefore consider it probable that the most important use of these ganglia is to constitute a principal source of the peripheric action of the vegetative influence of the nerves. It is well known that in every ganglion large and small ganglionic globules exist, which, as my own investigations have confirmed, for the most part lie distinctly, but near one another, in different groups, being separated by a number of nerve-fibres and by filaments spun around them, which latter were first observed by Remak, and the nature of which has not yet been exactly ascertained. I suspect that in some mode, not yet anatomically demonstrated with certainty, a connexion will be found to exist between these groups of small and large ganglionic globules, so that an irritation of the sensitive nerves is reflected in the ganglia on the adjoining smaller ganglionic globules belonging to the vegetative system; indeed, by irritating the skin, we can produce inflammation, but if the trunk of the nerve be divided, this no longer succeeds, as I have experimentally demonstrated twenty-six years ago,[1] and the vegetative power is lost. Consequently the transference of the centripetal force to the centrifugal in the sensitive nerves, or to the

[1] 'Observat. anat. Pathol.,' Amst., 1826, pp. 14, et seq. This experiment, too, is disputed by Snellen, who found that if the wounds were dressed with washed taffety, inflammation and suppuration supervened also in the paralytic foot. But it is certain that after division of the nerve the effect is less, as I saw distinctly in comparative experiments by Dr. Snellen with inflammation of the ear in rabbits, from which it appeared that in the sound ear the inflammation was much more violent, in the paralysed organ it was scarcely perceptible. See Snellen's dissertation above quoted, pp. 16 et seq. I must further observe that, as Remak has already discovered multipolar cells in the cœliac ganglion of the sympathetic, I have met with them both in the sympathetic and in the small ganglionic cells which are found in the sensitive cords of the spinal nerves with the larger cells, so that thus the suspicion that in the spinal ganglia sympathetic cells communicate with larger cells of the sensitive nerves is strongly confirmed.

vegetative filaments contained in them, appears to take place in the ganglia; that the ganglia do not serve for sensation is evident from the fact that the sensitive roots are not less sensitive above the ganglia, and that cutting off the ganglia does not destroy sensation in the central extremities of the roots.[1]

From all this I think we must infer that the atrophy in the ganglia of these nerves is directly connected with the atrophy of the different parts of the arm, the greater or less degree of which appears to depend rather on the amount of the loss of action which some fasciculi of nerve-filaments and their ganglionic globules have undergone.

The peripheric force and activity in the nerves, the mode of action of which is wholly unknown to us, has been very satisfactorily proved by Ludwig's extremely important experiments, showing that, on irri-

[1] Kölliker, who assumes the existence of both spinal and sympathetic filaments in the bones (*Mikroskopische Anatomie*, p. 340), lays it down that the sensitive filaments in the bones reflect their stimulus in the spinal cord to motor nerves, which thus react on the walls of the blood-vessels, and so regulate the condition of the vascular system. But it is far from being established that the peripheric action of the nerves consists solely in producing a movement, and therefore a contraction, of the muscular fibres, whether of other parts or of the blood-vessels; their action should in that case consist in causing the blood-vessels to contract, and so diminishing the vegetative function; indeed, Henle's very improbable hypothesis, that this irritation of the sensitive or centrally acting filaments produces paralysis of the vaso-motor nerves, and consequently dilatation of the vessels, is, in my opinion, inadmissible, and has already been amply refuted by various writers, and among others by Paget ('Lectures on Inflammation,' London, 1850, p. 55). In reference to this point it may be sufficient to observe, that in that case dilatation of the blood-vessels and inflammation should follow the division of a nerve, by which the motor filaments are certainly paralysed, whereas the contrary takes place. According to this hypothesis, too, in atrophy of the ganglia and of the spinal cord, that is, in paralysis of these nerves, atrophy could not ensue, as it did in my case. If we admit the theory of the mutual interchange of forces, as Grove has suggested for the physical forces (see Grove, 'The Correlation of the Physical Forces,' London, 1850), and Carpenter for the organic kingdom ("On the Mutual Relations of the Vital and Physical Forces," in the 'Philosophical Transactions of London,' 1850, pp. 727 et seq.), by which heat can be changed into motor force, or into electricity, or nerve-force passes into electricity in electrical fishes, or into motor force, &c., it can easily be explained why greater nervous action must produce an increased supply of juices and augmented activity of the parts. Although this theory, too, is attended with difficulties, Carpenter's essay, which contains many important views, deserves every consideration.

tation of a nerve of the salivary glands, therefore of a sensitive nerve, which contains vegetative filaments, the secretion in the gland is very considerably increased, without the pressure of the blood in the blood-vessels being at the same time and proportionately augmented;[1] whence it would seem that here a force is in operation very similar to galvanism, by which, too, currents of endosmose and exosmose are so much promoted.

But it may be objected to these arguments that, in my case, there was no independent atrophy of the nerve-ganglia, the latter only participating in the atrophy of the spinal cord, and that therefore the atrophy of the ganglia was probably the result of the earlier affection. It is precisely in reference to this remark that I think this case of especial importance; it, in fact, shows clearly that, as in our body all is united and combined into a whole, we should err in this instance also were we theoretically to separate parts which in nature are closely connected and capable of acting on one another. I have above adduced sufficient reasons for believing that the ganglia of the spinal nerves exercise an important influence upon nutrition and vegetative life; but that they also are again more or less dependent on the spinal cord, and are capable of receiving from it stimuli to action, is proved by this very case, where the unilateral atrophy of the spinal cord produced atrophy and diminution of action in these ganglia. But that these ganglia are not exclusively dependent on the spinal cord, but possess an activity of their own, is, in my opinion, clearly established, not only by the facts above adduced, but also by the so-called anencephali, where either the brain alone or

[1] Ludwig, "Neue Versuche über Beihülfe der Nerven zu der Speichel-secretion," in 'Mittheilungen der Zurich. Naturf. Gesellschaft,' No. 50, p. 27. It is well known that in this case the blood-vessels are dilated, and, in consequence of the acceleration of the current, the blood even passes in the arterial state into the veins, as Bernard has proved ('Comptes rendus,' 1858, Août, p. 245). The same was the case with the kidneys after irritation of the vagus subsequently to its transit through the diaphragm (Schmidt's 'Jahrbücher,' 1859, No. 10, p. 1).

For these reasons I suspect that there are two sorts of vegetative nerves—the known sympathetic filaments, which accompany the vessels, and whose function is to constrict the vessels, and others which, like the chorda tympani in the salivary glands, and the filaments of the nervus vagus in the kidneys, are distributed more in the parenchyma, and the irritation of which produces an augmented current of blood and effusion; these last must therefore play an important part in the theory of inflammation, where the explanation of the dilatation of the vessels presents so many difficulties.

the brain and spinal cord are wholly annihilated. It is well known that in such *lusus naturæ* the nerve-roots terminate in the dura mater, without being able to unite with the brain or spinal cord, because these parts have disappeared.[1] In several examples I have traced the central terminations of the nerves; in some the spinal cord ended at the superior cervical vertebræ; in these I found the Gasserian ganglion of the fifth pair uninjured beneath the dura mater, while the brain was wholly destroyed by the previous dropsy at an early period of fœtal life, without leaving a trace of its existence. It would appear that the sac, at an early date, while the skull and skin are still very weak, and can give no sufficient support, bursts, the cerebral membranes collapse, and all the cerebral matter in most cases completely disappears, so that often not a trace of it is to be met with.[2] The fifth pair of nerves was not connected with the medulla oblongata, the latter having been annihilated, and still the eyes and face were very well formed and developed. In other examples both the brain and the entire spinal cord were wholly absent, and nevertheless the body was well formed. In these cases, therefore, the brain or spinal cord could not act on or stimulate these ganglia, and the influence of the nerves, necessary for the growth of the several parts, must therefore have taken its origin from the ganglia themselves. The same subjects, however, distinctly exhibit also the influence of the spinal cord; for in as many of these examples as I have had an opportunity of examining, the fœtuses were, where the spinal cord was entirely absent, considerably smaller, less developed, and not so heavy; while in the anencephali, in which only the brain and a portion of the medulla oblongata are wanting, but the rest of the cord exists uninjured, the fœtuses are much heavier, and sometimes are even very largely developed, in consequence, as it appears to me, of the non-interruption of the exciting influence of the cord on these ganglia.[3]

[1] See several examples of this in W. Vrolik, 'Handboek der ziektek. ontleedkunde,' 1 Deel, pp. 472, &c. Geoffroy Saint-Hilaire gives a tolerably good drawing of these nerves in his 'Philosophie anatomique,' pl. 14, fig. 1. It is to be regretted that, in such important lesions, sufficient attention has not been paid to the condition of the ganglia.

[2] See also the above-quoted work of Professor W. Vrolik, 'Handboek,' pp. 491 et seq.

[3] Thus I found in all anencephali, where the spinal cord was absent, the roots of the nerves which terminate in the dura mater to be much slighter and atrophied. Having been accidentally present, some months ago, when my esteemed

It would lead me too far were I to endeavour still further to illustrate, by different examples of congenital malformations, the

friend, Professor W. Vrolik, received a fresh anencephalus, I suggested to him to inject the vessels and institute an accurate microscopic investigation. With this my friend readily complied, and has communicated the results of this and of a second case in the 'Nederlandsch Weekblad voor Geneeskundigen' of the 12th October, 1851, p. 410, which, however, I did not receive until after this essay had been sent in. The results of this investigation are, in many respects, important. In the first place, he found in the red sanguineous mass immediately investing the denuded base of the skull, and on which the malformed cerebral vesicles lie, the most beautiful muscular bundles, with the well-known transverse striæ. Among these fasciculi of muscular fibres, the central extremities of the cerebral nerves are recognisable, appearing in the centre, and being imperceptibly lost in the musculo-fibrous tissue which constitutes the principal mass of the base of the skull.

In the second place, he discovered that, with the exception of the olfactory nerve (which cannot properly be considered as a nerve-trunk), none of the other cerebral nerves exhibited the ordinary microscopic marks of primitive nerve-fibres, with the well-known double contour, and the distinction between the sheath and its contents, but appeared to consist of nothing else than connective tissue, with numerous nuclei and nuclear-fibres among it, *as if the entire structure of these nerves had remained in the neurilematic element.* In the Gasserian ganglion were, as usual, well-developed ganglionic cells, while the nerve-fibres running between them wanted all the ordinary marks of primitive fibres. But the remark is of importance, that this unnatural condition of the primitive fibres was confined to the central extremities of the cerebral nerves; in their peripheric distribution they exhibited the normal structure; at least, this was true of the facial, vagus and hypoglossal nerves, which he examined. The same was found to be the case in a second example, in which likewise the central terminations of the cerebral nerves were deficient in distinctly marked primitive fibres.

As a summary of his results, my friend gives "an unnatural structure of the central terminations of the cerebral nerves, which seemed to have remained in their neurilematic element, and therefore in the stage of sheath- or tube-formation of connective tissue, while the proper primitive fibres were wanting. In the brain, too, the primitive fibres were absent. Lastly, the primitive bundles of striated muscular fibres on the base of the skull are a proof of the independent formation of the parts of the body, each in the situation belonging to it. Unmistakeably," he continues, "they are the fibres of the frontal, occipital, and temporal muscles, which, although wanting the surface where they are usually expanded, have formed independently, and have mingled with the imperfect and unfinished cerebral elements. The coexistence of these perfectly striated fasciculi of primitive muscular fibres with the imperfect condition of the cerebral nerves, the central terminations of which we see appearing among the muscular fibres, might furnish an argument, if there were

importance, for nutrition and growth, of the presence of these ganglia; but I will add that, so far as my observations extend,

need of it, against the position of those who consider the growth of the muscular system to be dependent on the influence of the nervous system."

I regret that I cannot assent to these conclusions of my esteemed friend. In the first place, that these nerve-filaments which appeared as connective tissue should originally have existed under that form, I cannot admit. Why did they then exhibit themselves in their peripheric extremities, as well formed and filled with nerve-medulla? If we trace the first growth of the nerves, we shall find that they do not begin with the formation of connective tissue; on the contrary, the darker edge and the stronger sheath are as yet scarcely discoverable, as has been shown by Schwann ('Mikrosk. Untersuch.,' tab. iv, fig. 8), and subsequently by Kölliker ('Mikrosk. Anat.,' pp. 396 and 536 et seq.). It is, in my opinion, evident that the nerve-bundles are, in this case, in the first instance well formed, but that, by the rupture of the cerebral vesicle and the destruction of the brain, they are broken up and subsequently atrophied, with the loss of the contents of the nerve-tube, and the final contraction of the neurilema. It is particularly worthy of note that the ganglionic globules in the Gasserian ganglion were well formed, affording a proof that nervous influence was still capable of extending periphcrically from the globules after the destruction of the brain, since the extremities of the nerves were in general sound. In the spinal cord, on the other hand, which was not destroyed, the central terminations were also sound, so that, apparently, the destruction of the central terminations of the nerves was connected with the destruction of the centres themselves. I cannot admit that acrania might be a congenital malformation, and not dependent, like anencephalia, on a previous rupture of the cerebral vesicle, as my friend has represented in his 'Handboek der ziektekundige ontleedkunde' ('Manual of Morbid Anatomy'), D. i, p. 494; and in the 'Tabulæ ad illustrandam Embryogenesin,' tab. xliv, fig. 6, resting his views chiefly on the case of a fœtus of two months, where it was believed hydrocephalus could not have existed; but the brain is formed much earlier, and the fœtus represented in fig. 5, borrowed from Tiedemann, which was even smaller, still contained remnants of the burst cyst, and refutes such an opinion. Lastly, that the frontal, occipital, and temporal muscles should have formed independently, and, in the absence of the cranium, should have placed themselves arbitrarily on the base of the skull, appears to me to be wholly inadmissible; on the contrary, they show that they occur primarily in their natural situation, and on the collapse of the cerebral cyst are displaced from it; indeed, if each part forms independently in the situation to which it belongs, the rule does not hold good here, as the base of the skull is not the proper seat of these muscles. Further, the presence of the nerve-fibres, which are imperceptibly lost among the muscular tissue, clearly proves that, where muscular fibres exist, nerves belonging to them are also met with, especially as the peripheric extremities of the facial nerve are said to have been healthy; and this is not denied of the branches of the fifth pair.

In my opinion, Weber, in particular, has explained the point in question in a

wherever such a ganglion was either absent or destroyed, the part which should receive nerves from it has not been developed; it may therefore have begun to be formed in the original design of the fœtus, but for want of the vegetative influence of the nerve it has not grown and is absent, while, so far as I know, no example of the

tolerably decisive manner in his excellent essay, "Ueber die Abhängigkeit der Entstehung der animalischen Muskeln von den animalischen Nerven," in 'Verhandlungen der Gesellschaft der Wissenschaften zu Leipzig. Math. Phys. Classe,' 1849, iii St., pp. 136 et seq., fig. 1. He adds three cases (two observed by Alessandrini in Bologna), where a considerable portion of the spinal cord was wanting, and in which all the parts where no animal nerves existed, including the muscular fibres, were absent, so that while, for example, blood-vessels and lymphatics were present in the lower extremities, and the animal nerves which should accompany them were wholly wanting, the tendons of the muscles were present, but the muscular fibres which should occupy the space intervening were absent (l. c., p. 139)—a really remarkable phenomenon. On the other hand, the skin, cartilage, bone, synovial membranes, and aponeuroses, were present with the tendons, without animal nerves. Weber correctly remarks (l. c., p. 142), that the absence of the muscular fibres, under these circumstances, cannot be looked upon as the effect of a cause hindering their formation, by acting directly on the formative matter, as in that case neither could the development of the adjoining blood-vessels have taken place. Alessandrini's observation is of great importance in reference to this point, that the cord of the sympathetic ceased where there were no longer any spinal nerves, the sympathetic now consisting of trunks of the so-called splanchnic nerves, which were much thicker than usual; an unusually large solar plexus had formed around the abdominal aorta, and the plexuses and ganglia proceeding from it were developed in an extraordinary degree. From the mesenteric plexus very large plexuses, with interspersed ganglia, proceeded around the abdominal aorta, accompanying the branches of the latter, so that Alessandrini was able to trace them on the internal and external iliac arteries. He suspects that this unusual extension of the sympathetic occupied the place of the deficient cord of the nerve, and that hence branches arose for the parts in which the animal nerves were wanting (l. c., p. 197); it will therefore be necessary to investigate whether the skin and bones can be developed without sympathetic nerves.

Lastly, Alessandrini found in a pig, where the spinal cord terminated at the thirteenth spinal nerve, that in the first four caudal vertebræ a new portion of spinal cord again began, with nerves springing from it, and that muscles again appeared in the tail. This quite confirms what I formerly communicated in 1826, in my 'Observ. Anat. Path.,' p. 9, respecting two monsters, in one of which the lower part of the spinal cord was absent, and with it the nerves and muscles were also wanting, while in the other a fresh portion of spinal cord began, and with it nerves and muscles were again connected.

Weber further quotes the case of a child with acrania and absence of spinal cord, where, however, the muscles and also the nerves were normally present,

reverse has been met with, where a perfect ganglion existed, but the rest of the nerve or parts were absent. It is, however, of importance that acephalous malformations, where nerves are met with (in reference to which point no microscopic investigation has as yet been made), should be more closely examined in this respect, and that attention should also be directed to the often singular origin of these nerves in a kind of rudimentary spinal cord, with ganglia, which, so far as I am aware, has hitherto been overlooked.[1]

But in my case we find not only atrophy of the paralysed side, but, as I have above stated, hypertrophy of one half, and that the opposite half of the skull. This increased thickness of half of the skull in unilateral atrophy of the brain is of frequent occurrence, but is not constant; in twenty-seven cases of unilateral atrophy, where more or less mention is made of the skull, we find in ten instances increased thickness of the skull on the atrophied side of the brain; in other cases the cavity of the skull on the atrophied side was filled with serum, but the bone was not thickened.[2]

Even Gall observed that in case of atrophy of the brain the skull is thickened, and at the same time quotes examples of unilateral atrophy of the brain, where only one half of the skull was increased in thickness.[3]

and in which he even gives the measurement of the thickness of the nerves (loc. cit., pp. 144 et seq.).

He infers, in my opinion correctly, that the first formation of the spinal nerves is dependent on the existence of the spinal cord or a part of it; but if these are once formed, the cord is no longer necessary to the further nutrition and growth of the parts (loc. cit., p. 142). But where the formation of the spinal cord is interrupted before these nerves have been developed, they are not formed, and then the muscles are also absent (the force which continues to operate in these nerves after subsequent destruction of the cord, can, in my mind, be referred only to the ganglionic plexuses and their connexion with the sympathetic.)

There is much more which I must omit, and for which I must refer to Weber's important case and the conclusions he draws from it; the importance of the question alone induced me to enter into it at such length. Weber is, in my opinion, quite warranted in deducing the dependence of the muscles on the animal nerves.

[1] See, among others, different examples of this in W. Vrolik, 'Tabulæ ad illustrandam Embryogenesin,' Amst., 1849, tab. xlvi and xlvii.

[2] Obliquity of the skull often arises from dropsy within the head, which has already been observed by F. Meckel (see 'Handbuch der Path. Anat.,' Th. i, pp. 285.) Also W. Vrolik, 'Handboek der ziektek. ontl.,' pp. 522 and 533. Still I am not aware of any case of dilatation of the skull with atrophy of the brain.

[3] F. J. Gall, 'Sur les fonctions du Cerveau,' tome iii, pp. 89 et seq., and pp.

In fact, it cannot be denied that the skull often appears to adapt itself to the shape of the brain; my case affords a remarkable example of this. Thus, while the left side of the skull, so far as it covers the cerebrum, is increased in thickness, the reverse takes place beneath the tentorium, where the non-atrophied, sound hemisphere of the cerebellum is situated (Fig. I, *n*), here the left portion of the occipital bone is thinner, while the right, corresponding to the atrophied hemisphere of the cerebellum (Fig. I, *m*) is thicker, that is, on the side opposite to the thickened part of the cerebral portion of the skull.

In like manner, the left orbit (see Fig. I, *a*) is very convex and smooth, and has lost all the so-called impressiones digitatæ and juga cerebralia, which on the opposite side (*b*) are very distinct and sharply defined.[1] There can, however, be no doubt that these formerly existed here also, but that with the atrophy of the cerebral convolutions and of the brain itself they disappeared or ceased to grow, while the orbit became thicker and more convex. But what is the cause of this phenomenon, that the wall of the skull adapts itself to the brain, which the above does not explain? This phenomenon, moreover, is not constant: why, then, do so many cases of atrophy of one half of the brain, or, indeed, of both sides of the organ, occur, in which the skull was not thickened, but the space was filled with serum? It is indeed true that in atrophy of the eye the socket contracts; in dislocation of the femur out of the acetabulum the latter diminishes in size. Although I am not yet able to give any correct explanation of these singular changes in the bone, I suspect that different matters have been confounded. Wherever I found a skull increased in thickness the dura mater was very firmly adherent to it, and exhibited evident marks of chronic irritation, or, indeed, of

97 et seq. 'Anatom. et Physiol. du Système nerveux,' tome iii, p. 36, pl. liii.

[1] Gall also represents the same in his plate liii, where the orbit is evidently more convex on the atrophied side; the cranium is also thicker on the atrophied side, but the difference is not so great as in my specimen. But the petrous bone, too, appears arched, and a deep groove for the blood-vessel is likewise represented, as in my example for the arteria meningea media (see my Fig. I, letter *o*). The increased thickness, too, of the wing of the sphenoid bone, marked by me *c*, occurs in Gall's plate, but it is to be regretted that this drawing is little more than outline. However, this skull quite agrees in details with that in my case, though the several points are less strongly marked.

inflammation and dilatation of the blood-vessels. In my case of unilateral atrophy the same thing occurred; the cerebral membranes were very much thickened and inflamed upon the atrophied side, as I have represented in Fig. IV, *a*, *b*, *c*; the left inner surface of the skull, too (Figs. I, II), exhibits much deeper grooves for the blood-vessels, and the opening for the arteria meningea media (Fig. I, *r*) is wider than on the other side. I suspect that the dura mater has in these cases been irritated, and that thereby its external layer (which, as Arnold correctly remarks,[1] does not properly belong to the dura mater, but is a true periosteum) has been disposed to secrete an increased amount of osseous matter, and has thus caused the thickening of the skull. But if this irritation has not taken place, the space created by the atrophy of the brain is filled simply by an increased effusion of serum. In reference to this point, however, I cannot wholly forbear remarking that, as atrophy of the ganglia produces atrophy in the parts which receive nerves from them, the reverse has also occurred to me in several cases, where I have seen redness and inflammation of these ganglia combined with inflammation in the peripheric portion of the nerves belonging to them. I saw this particularly clearly in a case of pneumonia in a cow, where not only the vagus was inflamed in different places, but great redness of its ganglion and softness of its roots existed; and in a second case in a cow, where the tongue and larynx with the trachea were extremely inflamed, the mucous membrane of the last named exhibiting quite an extraordinary degree of thickness and redness; the nervus lingualis, plexus pharyngeus, and ganglion cervicale superius, were likewise swollen, very red, and even purplish. I have likewise seen great redness and swelling of the ganglion secundum vagi in severe pneumonia in a man. In post-mortem examinations of cases of inflammation, too little attention is paid to the state of these ganglia. I shall not here enter into a detailed

[1] F. Arnoldi, 'Annotationes anatom. de velamentis Cerebri,' Tur., 1838, cap. i, pp. 7 et seq. Sometimes these two laminæ are separated by an effusion of blood. Of this I have two remarkable specimens in my collection; in one the dura mater is separated over the greater part of one hemisphere of the brain into two layers, between which an extraordinarily great quantity of blood is effused; on the brain, which is flatly compressed by the blood, the arachnoid and pia mater are sound, and are not adherent to the dura mater. In this case, therefore, the dura mater is everywhere separated from the true periosteum internum, through which the arteria meningea media runs.

discussion of the question how far this affection of the ganglia may have been secondary or primary, but I think there are many reasons for admitting that a primary affection of these ganglia may sometimes arise, the result of which is a peripheric inflammation or change in the vegetative process,[1] although, perhaps, the affection in the ganglia may more frequently be secondary.

[1] This view is strongly supported by the remarkable experiments of Dr. Schiff, who succeeded in dividing the ganglion of the nervus vagus in rabbits, without injuring the accompanying motor branch of the recurrent nerve, the consequence being that no paralysis of the rima glottidis was produced. If he performed this operation on both sides, stagnation of blood and hardening or hepatization took place in definite circumscribed situations, particularly in the superior lobes of the lungs; if he operated only on one side, so that the life of the animal was prolonged, after from three to seven days white, grayish, firm indurations were developed in the upper lobe of the lung; these were, in fact, pulmonary tubercles; in this case, therefore, the tubercles were absolutely the result of a peripheric action of the nerve (see Schiff, "Ueber die Lungenveranderung nach Durchschneidung der pneumog. Nerven," in Rose und Wunderlich 'Archiv für Heilk. Phys.' 6 Jahrg., 8 Heft, pp. 769 et seq.) Since, as experience has taught me, we so often see pulmonary consumption occur in families some members of which are affected with insanity, so that I have often seen that the children who were spared from insanity were the victims of phthisis, and that the two diseases frequently alternate with one another, or coexist, the question has often suggested itself to me, whether we might not admit the existence of a phthisis excentrica, namely, one whose first cause is to be sought in an irritated condition of the medulla oblongata and the vagus, such as so frequently occurs in phthisical subjects. It must remain for further investigations to confirm this view; I shall only add that in such cases we often observe signs of spinal irritation in the neck, and that cupping and blistering often prove eminently useful.

It is particularly important to remark that patients who labour under softening of the brain often die of an affection of the lungs. Durand Fardel says: "Les accidents auxquels succombent les sujets affectés de ramollissement cérébral chronique, ou même aigu, ont presque toujours leur siège dans les poumons." ('Sur le ramollissement du Cerveau,' loc. cit., p. 434.) He deems the subject of sufficient importance to devote some chapters to it, and thinks especially that weakness of the circulation, and the consequent congestion and effusion in the lungs, occasion pneumonia. I was much struck with the fact, that among the cases of atrophy of the brain on record most of the patients died of phthisis, pneumonia, or enlargement of the heart; but I should doubt that a weakened circulation of blood through the lungs is of itself sufficient to explain this circumstance. See also Engel, in 'Prager Vierteljahrschrift,' vii Jahrg., Band iii.

The connexion between a morbid affection of the medulla oblongata and pulmonary disease has, in my opinion, not been sufficiently attended to. Thus, it appears from Table A, of epileptic patients, in my essay on the 'Spinal

The same is true of the thickening of the cranium, although it is difficult to decide whether in this case the irritation has extended to

Cord,' &c., p. 245 (vol. iv, New Sydenham Society's publications), that all the epileptic patients who had bitten the tongue died of phthisis, pneumonia, or marasmus (which is so often confounded with phthisis, and in which the lungs probably were not in a healthy state).

Engel, in his above-quoted essay in the 'Prager Vierteljahrschrift,' states that, among thirty-one cases of diseases of the brain, only in four was there no affection of the lungs or intestines; in fourteen of these cases there was inflammation of the lungs or pleura, in nine there was œdema of the lungs. Pneumonia occurred three times on the side opposite to the apoplexy, in which cases the extravasation was found on the outside of the corpus striatum and thalamus; once it occurred on the same side. In apoplexy of the base of the brain, in meningitis or hydrocephalus, the pneumonia was met with most frequently at the base and in the inferior lobes of the lungs. In inflammation of the left transverse sinus to the bulb of the jugular vein, the cerebellum being healthy, there was pleuritis of the left side, as he supposes, probably through affection of the vagus; I should rather suspect that in this case the left lateral column of the medulla had been affected and more or less paralysed, which would have been attended with increased immobility of the left side of the thorax. In an extremely remarkable case of spondylarthrocace of the first cervical vertebra, whereby the second was dislocated, and the odontoid process was changed into a sharp spine, compressing the left side of the medulla oblongata in the situation of the vagus, and producing semi-paralysis of both sides, as the pressure existed precisely on a level with the decussation, and thus injured some nerve-fibres which had already decussated, and some which were about to decussate, I found pleuritis and pneumonia, with little vomicæ in the left lung, the inferior lobe of which was soft and dark, without crepitation on section, and was therefore paralysed. A detailed description, with a drawing of this remarkable case, will shortly see the light. The experiments of Schiff on this subject are worthy of note. This observer found that, on section of the one half of the medulla oblongata beneath the calamus scriptorius, all the extremities, the head and ears, were warmer, in consequence of dilatation of the paralysed vessels on the injured side, but on the chest the opposite side was warmer, so that in this case the influence on the sympathetic or vaso-motor nerves was evident (M. Schiff, 'Untersuchungen der Physiologie des Nervensystems,' 1855, p. 207). On a former occasion Schiff found that after the division of one nervus vagus tubercles were developed in the superior lobe on the same side, and in the inferior lobe of the opposite lung, whence he infers that the two vagi are connected with one another by means of ganglionic cells in the lung, each vagus giving nerve-filaments to both lungs. (See Schiff, 'Ueber die Lungenveranderungen nach Durchschneidung des pneumogastrischen Nerven,' in Rose und Wunderlich's 'Arch. f. phys. Heilk.,' 1847, 6 Jahrgang, pp. 769 et seq.) Brown-Séquard, in his important essays, "Recherches sur la physiol., &c., de la protuberance annulaire," in the 'Journal de Physiologie,' communicates four cases of diseases of the protuberantia annularis and medulla oblongata in which diseases of the lungs were met

the dura mater, in consequence of the inflammation of the arachnoid, or whether the Gasserian ganglion participates in this inflammation of the hemisphere, and has peripherically excited to increased action the outer lamina of the dura mater or the periosteum internum, which receives chiefly vegetative filaments from the fifth pair of nerves.[1] The fact that the cerebral arachnoid was not connected with the dura mater and that the latter membrane exhibited nothing abnormal on the inner or concave side, appears to be in favour of the second view, as does also the increased thickness of the os jugale and of the left half of the lower jaw, which are under the influence of the fifth pair of nerves, and could not be directly affected by the disease of the brain. I regret, however, that in the dissection attention was not directed to the state of this ganglion, which, nevertheless, from the form of the skull, must have been altered in shape.

with. In one case a tumour pressed on the left side of the medulla oblongata and nervus vagus, with paralysis on the right side; there was pleuritis with tubercles in the lungs, without any difference being mentioned between right and left, to which point, perhaps, no attention was given (l. c., tome i, pp. 526 et seq.). In two other cases the affection was on the opposite side; in one instance the pressure was on the left side, on the medulla oblongata, and all the nerves to the foramen lacerum; here there was pneumonia, with false membranes on the right side (l. c., tome i, p. 532). In the second case there was exostosis on the right side, pressing on the cerebellum and the protuberance; the exostosis had grown from the petrous bone and the adjoining occipital bone, and must certainly have injured the medulla oblongata and nerves; there was pneumonia of the left side (l. c., tome ii, p. 123). In a case of a tumour on the left protuberance the patient died of phthisis (l. c., tome i, p. 760).

In two other cases, where the vagus appeared not to be affected, there was no thoracic lesion (tome i, p. 537, and tome ii, p. 127).

It is, however, important to observe, that inflammation of the lungs cannot always be traced to the direct influence of the nervous system; almost all these patients suffer in the last periods of their lives from dyspepsia, and sometimes from an almost total inability to swallow, in consequence of which food falls into the trachea, producing inflammation of the lungs, as I myself have more than once observed. This difficulty of swallowing occurred also in nearly all the cases communicated by Brown-Séquard, although inflammation of the lungs was not always present; while, very often, affections of the lungs and phthisis occur in patients affected with epilepsy and cerebral disease, where deglutition is not impeded.

[1] See Luschka's beautiful drawing of this, 'Die Nerven in der harten Hirnhaut,' Tübingen, 1850, tab. iii, page 98, where some osseous nerves from the Gasserian ganglion, which should be sympathetic filaments, are distinctly traced into the diploë of the cranium.

But this affords no explanation of the change of the orbit in blindness and atrophy of the eye, if there are no traces of irritation of the periosteum, nor does it account for the origin of the frontal sinuses. The bones are not, however, fixed and unchangeable parts; in them, too, incessant metamorphosis of tissue and alteration of parts go on, which appear to place themselves differently according to the varied impressions they receive; but of these changes we are in many cases unable to give any satisfactory explanation.

I have often observed that general thickening of the skull is of frequent occurrence, traces of previous chronic inflammation or irritation being at the same time present.

Thus, for example, in the thickened skulls which are frequently found in the insane, the dura mater adheres so firmly to the inside of the cranium that it is very difficult to separate it. In old men, in whom Gall describes thickening of the skull as a usual occurrence,[1] it does not by any means invariably occur. So far as my observations go, it is found only where chronic affections have existed, in consequence of which atrophy of the brain has at length set in, with more or less serous exudation and traces of chronic meningitis; that is, under the same circumstances as exist in unilateral atrophy.

[1] 'Anat. et Phys. du Syst. Nerveux,' pl. xlv, tome iii, pp. 19 et seq.

Difference between the right and left side of the parts of the body in Atrophy of the Left Hemisphere of the Cerebrum.

Right side.	Millimètres.	Left side.	Millimètres.	Difference.
Brain.		*Brain.*		
Length of the hemisphere from the anterior to the posterior lobe	163	......	136	27
Length of the anterior lobe to the fissure of Sylvius	61	......	43	18
Length of the posterior lobe to the anterior extremity of the middle lobe	125	......	111	14
Thickness of the convolutions on the upper surface of the hemisphere beneath the frontal bone	11	......	3	8
On the posterior lobe they were more equal on the two sides.				
Breadth of the crura cerebri at the place of exit of the third pair of nerves ...	18	......	11	7
Corpus mamillare, across	6	......	3	3
Anterior tubercula quadrigemina	10	......	9	1
Longitudinal diameter of the pons varolii from the place of insertion of the sixth pair of nerves to the anterior margin ...	25	......	22	3
From the middle of the anterior edge of the pons varolii across to its entrance into the cerebellum	31	......	30	1
Corpus pyramidale at the edge of the corpus olivare	12	......	9	3
Cerebellum.		*Cerebellum.*		
Longitudinal diameter from the anterior edge of the flocculus, at the entrance of the pons, to the posterior margin of the hemisphere, not far from the vermis ...	48	......	56	8
Transverse diameter from the margin of the medulla oblongata to the outer edge of the hemisphere	45	......	52	7
Vertical diameter from the tonsillæ to the upper surface...	33	......	45	12
Arm.		*Arm.*		
Length of the clavicle	129	......	140	11
Circumference of the clavicle two and a half centimètres from the sternum	28	......	36	8
Length of the scapula from the upper margin to the inferior angle...	136	......	152	16
Breadth of the scapula under the spine from the margin of the articulation to the posterior dorsal side	88	......	97	9
Length of the spina acromialis from the posterior dorsal edge to the extremity of the process	120	......	132	12
Length of the humerus	293	......	319	26
Circumference of the humerus at the point of the insertion of the deltoid	48	......	59	11
Length of the radius	197	......	215	18

Right side.	Milli-mètres.	Left side.	Milli-mètres.	Differ-ence.
Arm.		*Arm.*		
Circumference of the radius beneath the tuberosity for the insertion of the biceps	34	......	40	6
Length of the ulna	220	......	235	15
Circumference of the ulna at the same level as that of the radius	41	......	46	5
Length of the hand from the upper edge of the os naviculare to the point of the middle finger...	179	......	179	—
Circumference of the os metacarpi of the middle finger...	22	......	27	5
Leg.		*Leg.*		
Length from the upper margin of the great trochanter of the femur to the inferior edge of the external condyle...	389	......	404	15
Circumference of the middle of the femur.	70	......	75	5
Length of the patella...	34	......	40	6
Breadth of the patella	34	......	38	4
Length of the tibia from the internal condyle to the lower edge of the malleolus internus	345	......	363	18
Circumference of the tibia at the point of insertion of the ligamentum patellæ ...	95	......	110	15
Length of the fibula	319	......	336	17
Length of the foot from the point of the heel to the extremity of the great toe, equal on both sides.				
Length of the os metatarsi of the great toe	54	......	57	3
Circumference of the middle of the same bone	43	......	41	2
Trunk.		*Trunk.*		
Length of the first rib, along the outer margin, from the posterior attachment to the vertebræ to the cartilage	128	......	148	20
Length of the seventh rib, measured in the same manner...	284	......	290	6
Length of the eleventh rib	202	......	204	2
Vertical diameter from the highest point of the os ilium to the inferior margin of the tuber ischii	204	......	210	6
Height from the upper margin of the pubis above and near the acetabulum to the inferior edge of the tuber ischii	93	......	97	4
Length from the antero-inferior spine of the ilium to the posterior tuberosity behind the sacrum	131	......	136	5
Muscles.		*Muscles.*		
Breadth of the deltoid	82	......	98	16
Breadth of the biceps	20·5	......	24·5	4
Breadth of the flexor carpi radialis	13	......	23	10
Nerves, see the table in the text at p. 141.				

EXPLANATION OF THE PLATES.

Fig. I.

Base of the skull, the left half of which, A, B, C, is much thicker than the right, D, E, F.

a. Strongly arched and thickened left supra-orbital plate of the frontal bone, quite smooth, without a trace of impression of the brain or of waving of the bone.

b. Right supra-orbital plate, with numerous impressiones digitatæ.

c. Left ala of the sphenoid bone, with a convex, uneven surface sloping towards the base.

d. Right wing of the sphenoid bone, the surface of which is concave, with numerous impressiones digitatæ.

f, g. Surface of the petrous bones.—*f.* Convex, raised surface of the left petrous bone, with a well-marked fissure for a vessel, without any trace of the arching of the semicircular canal. *g.* Sound petrous bone of the right side, through which the semicircular canal distinctly glimmers.

h, i. Situation of the foramen jugulare, covered on both sides by a spine of the petrous bone.—The left, *h*, is much narrower than the right, *i*.

k, l. Processus clinoidei anteriores.—*k.* The left, much thicker, more arched, and projecting more backwards than on the right side, causing them to stand obliquely.

m, n. Cavities in the occipital bone for the cerebellum.—*m.* The right depression is much smaller, bounded towards the occipital foramen by a deep groove (the sinus occipitalis inferior), which does not occur on the opposite side The bone is here thicker; behind this depression a new, white, osseous plate is seen in the transverse sinus. . The larger and wider depression for the left lobe of the cerebellum.

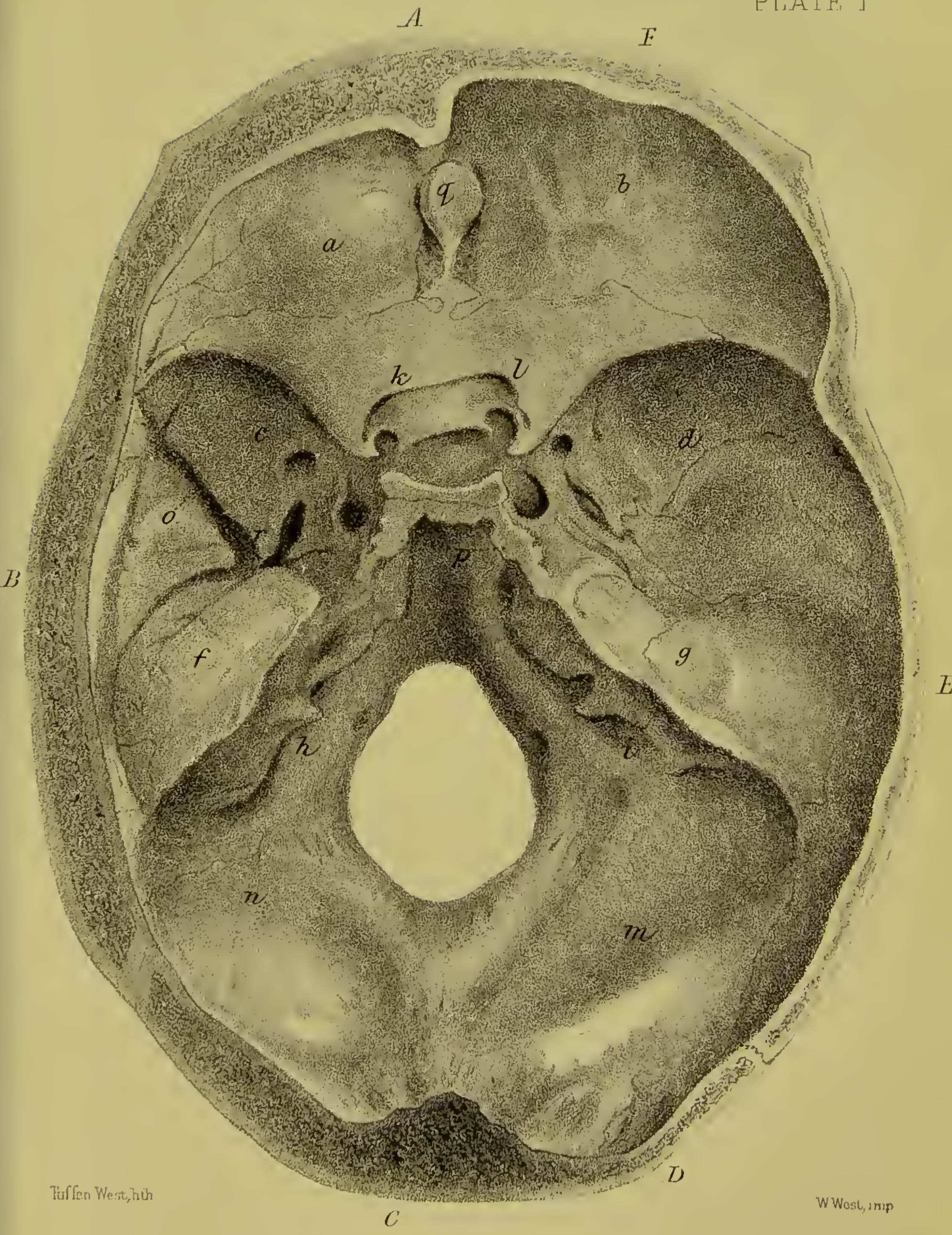
o. Very well-marked and deep groove for the arteria meningea media.

p. Processus clinoidei posteriores. On both sides a new bony lamina runs from the os petrosum towards the clinoid processes, and is separated from them by a transverse suture above *p*.

q. Crista galli, very large and high, quite bent over towards the left side, and there covering the cribriform plate.

r. Opening for the dilated left arteria meningea media.

PLATE I



Tuffen West, lith

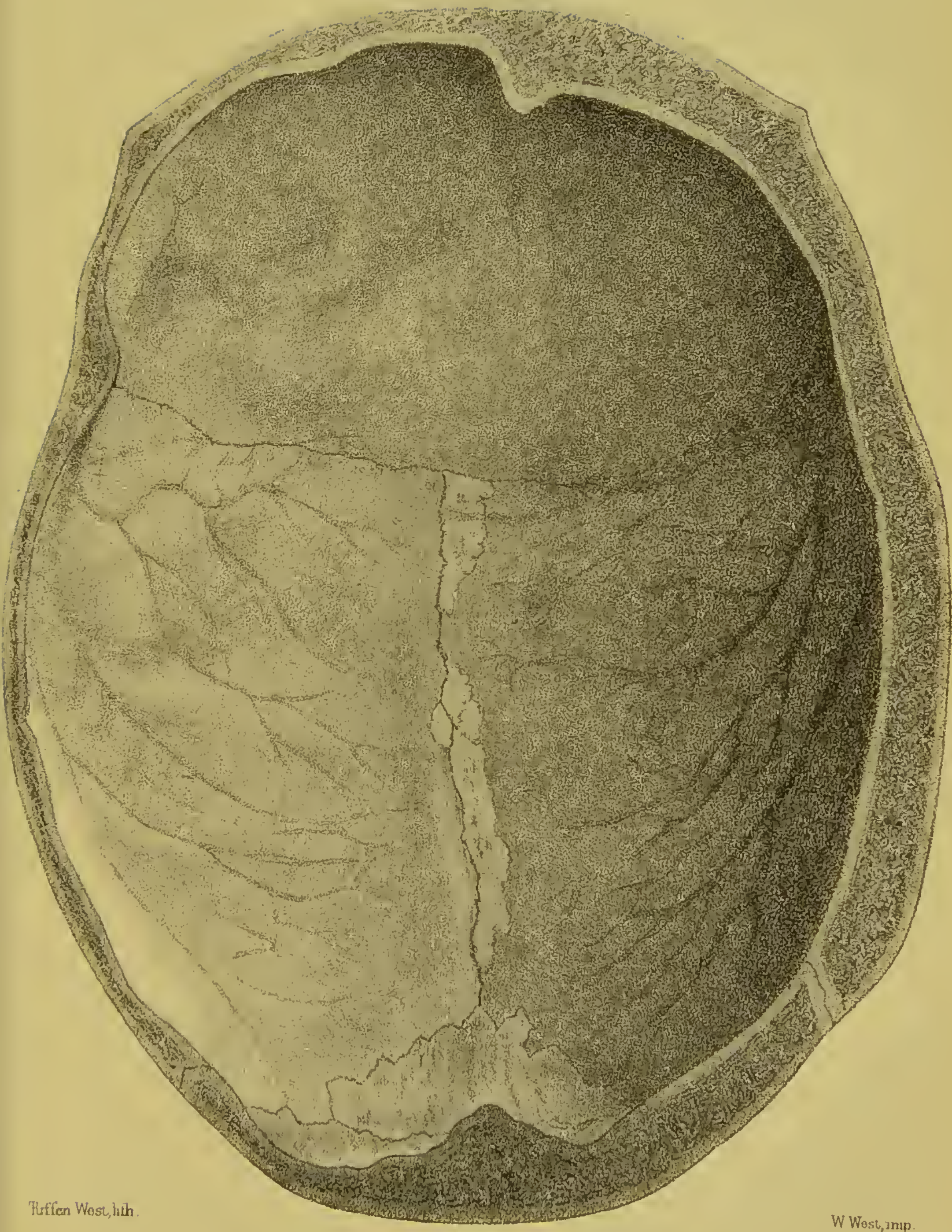
W West, imp

Fig. II.

Arch of the skull, from within.

Without the use of letters, the deeper impressions of the enlarged blood-vessels on the thickened side of the skull will be plainly seen, together with the new, white, osseous laminæ along the longitudinal sinus. The differeuce which must have existed between the contents of the two halves of the cranium will at onee strike the observer.

PLATE 2.



Tuffen West, lith.

W West, imp.

Fig. III.

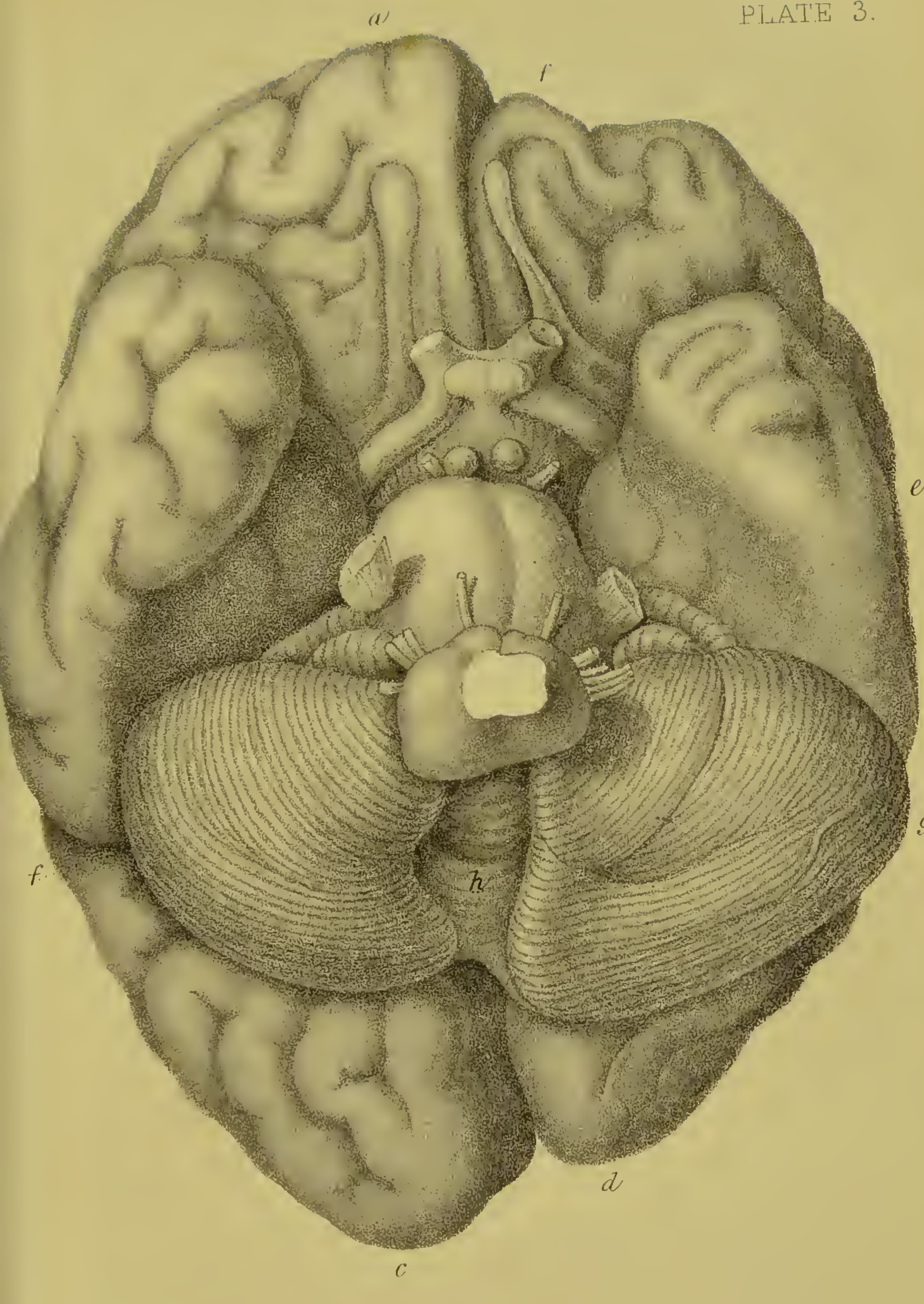
Base of the brain.

a, *b*, *c*. Right hemisphere of the brain.

d, *e*, *f*. Left, smaller and atrophied, hemisphere of the brain. The convolutions are here much diminished, and have nearly disappeared.

f', *g*, *h*. Cerebellum.—*f'*. Right and smaller lobe. *g*. Left and larger lobe of the cerebellum. *h*. Processus vermiformis, rendered quite oblique by the atrophy of the right lobe.

PLATE 3.



Tuffen West, lith

W West, imp

Fig. IV.

Section of the cerebrum and its ventricles.

a, b, c. Left atrophied half of the brain. The thickening of the arachnoid and pia mater is distinctly visible, and the smallness of the divided convolutions, when compared with those of the other side, is very striking.

d, e, f. Right and sound half of the brain. The convolutions are here larger, and the white medullary matter around the cavities is much thicker than on the opposite side.

g, h, i, k, m. Ventricles of the brain.—*g, h, i.* Left ventricle, in which, at *g*, the small, curved corpus striatum, and especially the much atrophied thalamus and the thickened blood-vessel between both, are visible. *h.* The very much dilated entrance into the inferior ventricle. *h, i.* The posterior dilated ventricle, in which the impressio digitata is very strongly marked. *k, m.* Right, sound ventricle. *l.* Right and larger thalamus, behind which the oblique pineal gland and unequal tubercula quadrigemina are visible.

PLATE 4.

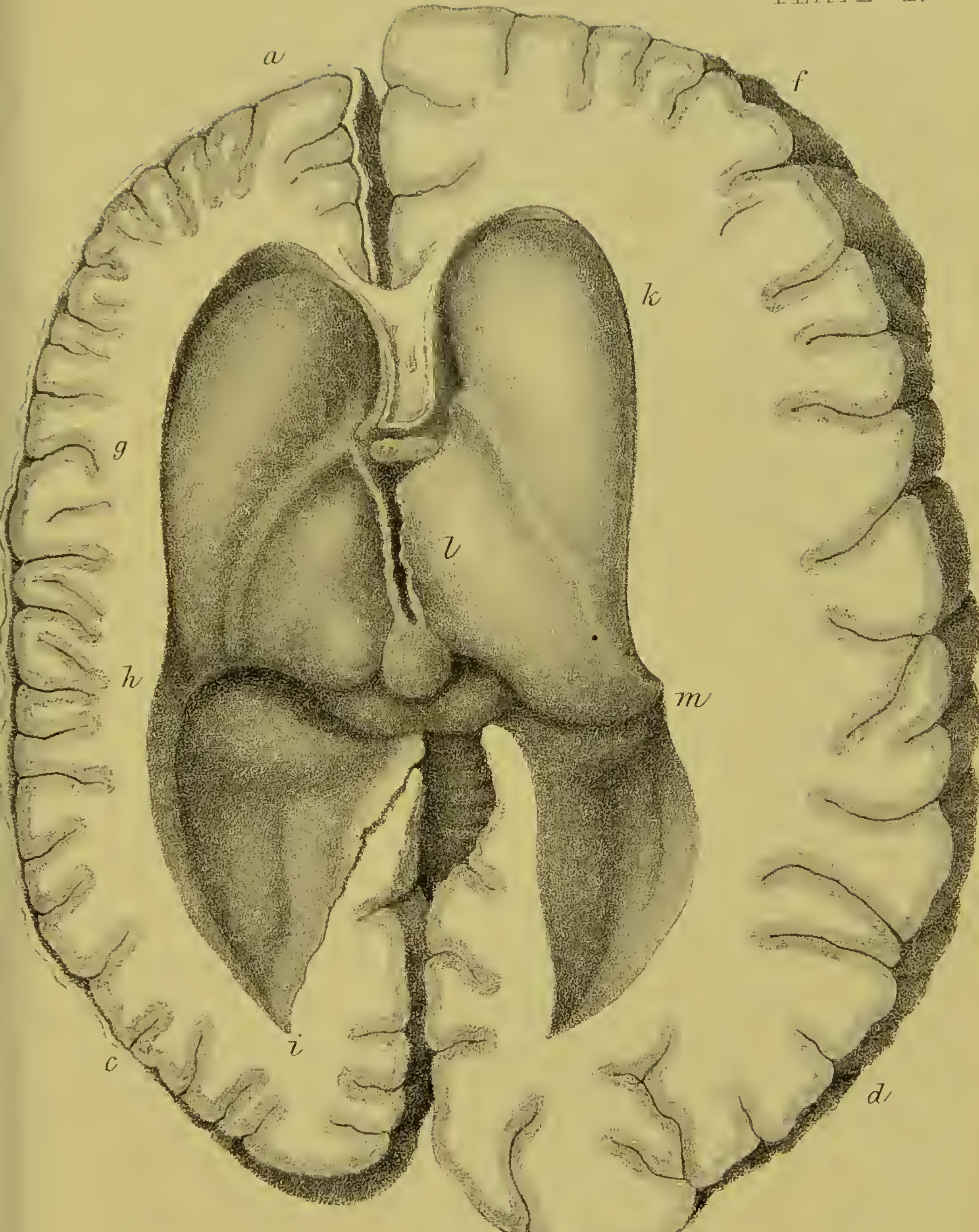
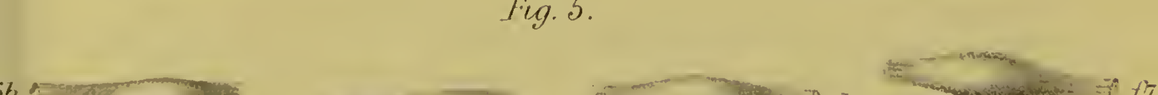


Fig. 5.



W.West, imp.

Fig. V.

Ganglia of the fifth, sixth, and seventh nerves.

a, b, c. Ganglia of the right or atrophied side, which are much slighter than those of the opposite or left side.

d, e, f. Ganglia of the left side, of which *f* is the thickest.

ON THE

IMPORTANCE AND VALUE

OF

ARITHMETIC MEANS;

WITH ESPECIAL REFERENCE

TO RECENT PHYSIOLOGICAL RESEARCHES ON THE DETERMINATION OF THE INFLUENCE OF CERTAIN AGENCIES UPON THE METAMORPHOSIS OF TISSUE;

WITH

RULES FOR ACCURATELY ESTIMATING THE SAME.

BY

PROFESSOR RADICKE,

OF BONN.

TRANSLATED BY

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THE NEW SYDENHAM SOCIETY,

LONDON.

MDCCCLXI.

PREFACE.

A FEW words are, perhaps, necessary to explain why it has been thought desirable to present the following papers to the English medical reader in their present form. At a time so prolific in research of all kinds as the present, and when the aid of numerical comparison is so frequently invoked, not only in the domain of physiology and therapeutics, but also in that of practical medicine, it is imperatively requisite that we should know exactly the nature and amount of the assistance which such an aid is capable of affording. If it were necessary to add to the illustrations which Professor Radicke has given in the first of his papers of important generalisations founded upon an utterly insufficient and often inaccurate basis, there would be little difficulty in finding them in the English medical literature of the last few years. The service, therefore, which Professor Radicke has done to the cause of science, in thus calling attention to a growing evil, as well as in pointing out its remedy, can hardly be over-estimated, and his able and exhaustive discussion of the subject will, it is to be hoped, do something, in his own words, "to stem the stream of baseless and, to a great extent, erroneous doctrines by which medical science threatens to be overwhelmed." Statistics are a most

effective instrument of research when rightly used; but, like other edged tools, in unskilful hands they are as likely to do hurt as good.

It has been thought desirable that Professor Radicke's paper should be brought under the notice of the profession in England through the medium of the influential circulation of the New Sydenham Society for two reasons—first, because there is no treatise readily accessible to the majority of readers in which the subject is so compendiously discussed, and in a manner so adapted to the capacities of those who have not enjoyed the benefit of special mathematical training, as in this paper; and secondly, because in the form in which it was originally published it could only be consulted with difficulty even by those who possessed the resources of the metropolis, whilst for those who resided in the country it was almost unattainable. At the same time, whilst the council of the Sydenham Society were taking steps to circulate the cautions of Professor Radicke, they felt that they would hardly be doing justice to the controversy which it originated if they entirely ignored the attacks of which it was made the subject. So much of the papers, therefore, in which these were contained has been appended as is necessary to exhibit the grounds upon which Professor Radicke's statements could be impugned, as well as to render intelligible the defence with which he has thought it desirable to supplement them.

F. T. B.

ON THE

IMPORTANCE AND VALUE

OF

ARITHMETIC MEANS.[1]

Section I.—*Introduction.*

It is due to my friend, Dr. Boecker, to state that the present paper has been undertaken at his instance. Dr. Boecker has on several occasions submitted to me recent papers of a pharmacological nature, with the view of obtaining from me a confirmation of his opinion, that great want of caution is frequently exhibited in drawing inferences from observations; and he at length has requested me to assist him in stemming the stream of baseless and, to a great extent, erroneous doctrines which daily threaten to overwhelm medical science.

With this object in view, I propose, as one who is acquainted with mathematics and physics, to whose province the treatment of questions of this kind belongs, to provide medical men with a ready test by which they may themselves try the accuracy of the conclusions at which they may arrive.

I cannot, however, promise to give them an absolutely unvarying and accurate test, since for the cases contemplated in this paper such

[1] It is hardly necessary to remark that the term "mean," as employed here and throughout the paper, is used as the equivalent of "mean value," than which it has the advantage of being more compendious.—*Translator.*

a test does not, and probably never can, exist: all that I can offer is the test which I myself consider a sufficient one in investigations of this kind, and on which my requirements will be based.

It is well known that the process usually employed in determining the influence of any given agency, such as nutritive or medicinal substances, baths, &c., upon the metamorphosis of tissue is as follows: An individual is subjected, during a certain number of days continuously, to the influence of the agency in question, with the observation of a manner of life in other respects as uniform as possible, and the daily excretions—more especially, and often exceptionally, the urine—are determined both with reference to their aggregate amount and to the quantities of their most ordinary chemical elements. The data obtained are then compared with those of a second series of days, which may precede or follow the first, and in which the same mode of life is observed, with the exception of the absence of the agency under investigation. The comparison is, however, usually limited to placing the Arithmetic Means obtained from the numbers of the first series of days by the side of those deduced from the second series, and to concluding, in accordance with the preponderance of the first Means over the second, or *vice versâ*, that the agency in question increases or diminishes the excreta under observation. The urinary quantities are generally determined in cubic centimètres, the chemical elements in milligrammes,[1] and a difference in the last one or two figures of these is considered as amply sufficient for the basis of any conclusion. The defect of this method of proceeding lies in the fact that those who employ it do not clearly comprehend the real significance and value of Arithmetic Means, and, as a consequence, have attributed to them a value which in such applications as these they do not actually possess. It will, therefore, be desirable to make some preliminary observations on the different acceptations in which the term "Arithmetic Mean" may be received, according to the variety of case to which it may be applied.

Section II.—*Of Mean Values in General.*

We generally understand in ordinary matters by the term "mean value," the Arithmetic Mean exclusively; and it is therefore necessary

[1] This of course applies to the Continent: the decimal system has not yet made such progress in England as to allow of its universal substitution for the old method of notation, though it is to be hoped that the time is not far off when such will be the case.—*Translator.*

to point out that, in mathematics, this term is employed in a wider sense. That one of any series of numbers is called their *mean value,* which, lying between the greatest and least of those numbers, stands in a definite dependence upon the whole of the series. We may, therefore, employ several kinds of means, accordingly as we may find it desirable to exhibit that dependence in one way or another; and the special nature of each case must determine which kind of mean shall be employed. Those which we most frequently meet with are the following:

(*a*) *The Arithmetic Mean.*—The relation of this Mean to its series of numbers is, as is well known, such, that the sum of the differences of those numbers which are greater is exactly equal to the sum of those which are less than it. The Arithmetic Mean of the series 3, 6, 9, 4, 13, for instance, is 7; the numbers which are greater than 7 (i. e., 9 and 13) differ from it respectively by 2 and 6; those which are less than 7 (i. e., 3, 6, and 4), on the other hand, respectively differ from it by 4, 1, and 3; and the sums of the two series of differences, viz. $2 + 6$, and $4 + 1 + 3$, are equal in value. If we assume that the differences of the greater numbers are negative, then the algebraic sum of *all* the differences (in this case $-2-6+4+1+3)=0$.

(*b*) *The Geometric Mean.*—If n represents the number of figures in the series, the Geometric Mean represents the n^{th} root of the product of these figures. The Geometric Mean, for instance, of 3 and $12 = \sqrt{3 \times 12} = \sqrt{36} = 6$; whilst the Arithmetic Mean of the same numbers is $7\frac{1}{2}$. The Geometric Mean also of 3, 9, and 8 $= \sqrt[3]{3 \times 9 \times 8} = \sqrt[3]{216} = 6$; whilst the Arithmetic Mean of those numbers is $6\frac{2}{3}$. The reason why the result always actually turns out a mean number, and why this number also differs but slightly from the Arithmetic Mean, is, that the extraction of the root is an operation which is antagonistic to repeated multiplication, and therefore the alteration produced by multiplication is to a certain extent neutralized by it. The second of the above Means, for instance, $\sqrt[3]{3 \times 9 \times 8}$, is obviously greater than $\sqrt[3]{3 \times 3 \times 3}$ (i. e., greater than 3), but is less than $\sqrt[3]{9 \times 9 \times 9}$ (i. e., less than 9), and consequently lies between 3 and 9 as extremes. If we substitute 9 for the third of these numbers, viz., 8, we get $\sqrt[3]{3 \times 9 \times 9}$, and, therefore, a number which is nearer to $\sqrt[3]{9 \times 9 \times 9}$ than to $\sqrt[3]{3 \times 3 \times 3}$ (i. e., nearer to 9 than to 3).

And if we substitute for 8 the numbers which descend successively from 9 to 3, we get numbers (viz., $\sqrt[3]{3 \times 9 \times 9}$, $\sqrt[3]{3 \times 9 \times 8}$, $\sqrt[3]{3 \times 9 \times 7}$, &c., &c.) which diverge gradually more and more, as in the case of the Arithmetic Mean, from 9, and approach nearer and nearer to 3; so that the Geometric can never differ greatly from the Arithmetic Mean.

(*c*) *The Harmonic Mean.*—In order that I may define this Mean as clearly and succinctly as possible, I must remind the reader that by *the reciprocal value* of any number is understood that figure which is obtained when we make the number in question the divisor of unity. Consequently, as an illustration, the reciprocal value of $9 = \frac{1}{9}$; of $\frac{4}{5} = \frac{1}{\frac{4}{5}} = \frac{5}{4}$; hence the reciprocal value of a fraction is that fraction inverted. With this explanation, the Harmonic Mean may be defined as *the reciprocal value of the Arithmetic Mean of the reciprocal values of the given numbers.* According to this definition, we obtain the Harmonic Mean of 3 and 12, for instance, if we take the Arithmetic Mean of $\frac{1}{3}$ and $\frac{1}{12}$ $\left(\text{viz. } \frac{\frac{1}{3} + \frac{1}{12}}{2} = \frac{5}{24}\right)$ and then find its reciprocal value (viz., $\frac{24}{5} = 4\frac{4}{5}$). The Harmonic Mean of 3 and 12, therefore, is $4\frac{4}{5}$. In the same way, to obtain the Harmonic Mean of 3, 9, and 8, we take the Arithmetic Mean of $\frac{1}{3}$, $\frac{1}{9}$, and $\frac{1}{8}$ $\left(= \frac{\frac{1}{3} + \frac{1}{9} + \frac{1}{8}}{3} = \frac{41}{216}\right)$, and find its reciprocal value, viz., $\left(\frac{3}{\frac{1}{3} + \frac{1}{9} + \frac{1}{8}} = \frac{216}{41} = 5\frac{11}{41}\right)$. Why we always, as a result of this process, arrive at a mean number will be clear when it is observed that the *reciprocal value of the reciprocal value* of any number is the number itself again; for instance, the reciprocal value of 8 is $\frac{1}{8}$, of which the reciprocal value is again $\frac{8}{1}$, or 8. The changes which the numbers operated on suffer in obtaining their Harmonic Mean, through the process of taking their reciprocal value, are again to a certain extent neutralized by the repetition of the same process; or, to express it more definitely, when we take the Arithmetic Mean of the reciprocal values of the given numbers, we obtain the reciprocal value of a mean number; and

when we again take the reciprocal value of this latter, we obtain the mean number itself. Thus, if we employ the second of the above illustrations as an example, it is evident that $\frac{1}{3} + \frac{1}{9} + \frac{1}{8}$ is intermediate to $\frac{1}{3} + \frac{1}{3} + \frac{1}{3}$ and $\frac{1}{9} + \frac{1}{9} + \frac{1}{9}$, and consequently that the Harmonic Mean, $\frac{3}{\frac{1}{3} + \frac{1}{9} + \frac{1}{8}}$, must fall between $\frac{3}{\frac{1}{3} + \frac{1}{3} + \frac{1}{3}} = \frac{1}{\frac{1}{3}}$ and $\frac{3}{\frac{1}{9} + \frac{1}{9} + \frac{1}{9}} = \frac{1}{\frac{1}{9}}$, i. e. between 3 and 9.

Moreover, it will be evident that when (as was done previously in explaining the Geometric Mean) we leave the first two of the three numbers, 3, 9, and 8, unaltered, whilst we substitute for the last a value which descends gradually from 9 to 3, the result must also approximate by degrees from 9 to 3, so that the Harmonic Mean must pass through the same course as the Arithmetic Mean does when treated in a similar manner.

(*d*) A fourth kind of Mean, which has a particular interest for the present investigation, inasmuch as it will be necessary hereafter to employ it, I shall call the *Quadratic Mean*, for want of a better name, since it has had none hitherto. This Mean is equivalent to *the square root of the Arithmetic Mean of the squares of the given numbers*. Thus, for instance, the Quadratic Mean of 3 and 12 = the square root of the Arithmetic Mean of 3^2 and 12^2, i. e. of $\frac{9 + 144}{2}$, or of $\frac{153}{2}$, or of 76.5, and its value is consequently $\sqrt{76.5} = 8.75$. In the same way the Quadratic Mean of 3, 9, and 8 $= \sqrt{\frac{9 + 81 + 64}{3}} = \sqrt{51.33} = 7.16$. The result is on each occasion a mean number because the alterations which the given numbers suffer at the commencement, in the process of squaring, are to a certain extent compensated in the extraction of the square root which follows; or, to speak more exactly, the Arithmetic Mean of the squares of the given numbers is obviously equal to the square of a mean number and the square root of this is consequently the mean number itself. For example, the Quadratic Mean of 3, 9, and 8, viz. $\sqrt{\frac{3^2 + 9^2 + 8^2}{3}}$, is obviously less than $\sqrt{\frac{9^2 + 9^2 + 9^2}{3}}$, and greater than $\sqrt{\frac{3^2 \quad 3^2 + 3^2}{3}}$,

and consequently is intermediate to the extremes of the numbers in question (since $\sqrt{\frac{9^2+9^2+9^2}{3}} = \sqrt{9^2} = 9$, and $\sqrt{\frac{3^2+3^2+3^2}{3}} = \sqrt{3^2} = 3$). Moreover it must be remarked, as in the case of the Geometric Mean, that if the two extreme numbers are left unaltered, the result will be nearer to 9 or 3 in proportion to the greater or lesser amount of the third number.[1]

It must be noted here that it may be exactly and mathematically demonstrated, that the Quadratic always somewhat exceeds the Arithmetic Mean, and that to an amount proportionate to the inequality of the given numbers. The first of these assertions is illustrated in the two examples given above, in which the Quadratic Means were 8.75, and 7.16, whilst the Arithmetic Means were only $7\frac{1}{2}$ and $6\frac{2}{3}$. As an illustration of the second, viz. that the divergence increases with the inequality of the numbers, the following will serve: The series 3, 2, 9, 10, and 3, 5, 6, 10, give the same Arithmetic Mean, viz. 6. On the other hand, the first of them, which contains the more unequal numbers, gives the Quadratic Mean of 6.96, whilst that of the second is only 6.52.

The more equal the numbers, the nearer does the Quadratic approach to the Arithmetic Mean; and when they are quite equal, it exactly coincides with it. This character, however, is not peculiar to the Quadratic Mean; it will have been seen, by the two examples given above, that it is possessed also by the Geometric and Harmonic Means, and it occurs, as a necessary sequence of the general idea of a Mean, with all kinds of Means. The Mean of the numbers 9, 9, 9, &c., is 9, of whatever kind it may be.

[1] Although reference will be made hereafter to this peculiarity of the Quadratic Mean, this may not be an unfit place to prove, for those readers who are acquainted with Algebra, the single case at least where the inequality lies in two numbers. If the numbers a and b occur in a series, their arithmetic mean is not affected by substituting for them $a+c$ and $b-c$. Let us suppose, now, that $a > b$, then $a+c$ and $b-c$ are more unequal than a and b, and the Quadratic Mean will, in conformity with the rule, be greater in the second case than in the first, so soon as $(a+c)^2 + (b-c)^2 > a^2 + b^2$. But $(a+c)^2 + (b-c^2) = a^2 + 2ac + c^2 + b^2 - 2bc + c^2 = a^2 + b^2 + 2c\,(a-b) + 2c^2$; consequently, since $a > b$, and also $a-b$, as well as $2c(a-b)$, is positive, in reality $(a+c)^2 + (b-c)^2$ exceeds a^2+b^2 by the positive quantity $2c\,(a-b) + c^2$. In a similar manner may we prove the accuracy of the proposition for the case where the series includes more than two unequal numbers.

No further reference will be made in the following remarks to the Geometric and Harmonic Means; but it appeared to me not unsuitable to describe them, in order to show the reader that it is possible to employ a considerable variety of Means, some of which are preferable for one purpose, and some for another.

I will now return to those which interest us most—the Arithmetic Means — in order to inquire whether and to what extent they exhibit what, in the cases under consideration, they ought to represent, and how far they are applicable to the comparison we are now discussing.

So far as the cases are concerned in which the Arithmetic Mean may be employed, this latter may represent either (1) a pure average, or (2) the probable value of a definite and *fixed* quantity, or (3) the probable value of a *variable* quantity estimated in its mean condition. The Means with which we have to do in this place belong chiefly to the third of these categories; but inasmuch as they are to a certain extent a combination of the two other varieties, it will be advisable to treat separately of the two elements of which this combination is composed, and for that purpose to make some inquiries into their nature.

Section III.—*The Arithmetic Mean as a pure Average.*

I call the Mean "a pure average," when the numbers from which it is deduced are regarded as exact, and are either independent of one another, or their dependence, if such exist, is not taken into consideration.

The average of the ages of a party of individuals may serve as an illustration. Suppose, for instance, that this is 72 years; it does not follow, therefore, that any one person in the company is exactly of this age, or even that the ages of the majority approach to it; but, as the character of the Arithmetic Mean, given in the previous paragraph indicates, that the sum of the ages of those persons who are above 72 is equal to the sum of the ages of those who are younger than 72; hence all of them would be of the mean age, if the older could transfer to the younger so much of their superiority in years as would make them all equal. If, now, we wish to institute a comparison with a second and equally numerous company of persons, whose average age is 67

years, we are not justified in concluding from this latter fact that any of the members of the first series are older than those of the second, since the reverse might easily be the case. All that is certain is, that *the aggregates of the ages* of the two companies would bear to one another the relation of 72 to 67.

In the same sense the mean temperature of any one day indicates a simple average. Thus, suppose that this is 12° [Reaumur, equal to nearly 60° Fah.]; this means that the aggregrate of degrees of temperature by which the warmer hours of the day have exceeded 12° is equal to the aggregate of those hours in which the temperature fell below that point. And if on a second day the mean temperature indicates 14°, it must not be inferred as a consequence that the heat on that day has been greater than on the other; for a day on which the hours at noon were very hot, and the early ones very cold, might have a much lower mean temperature than one in which the general heat was less excessive, and the fluctuations more moderate. All that is certain is that, if the heat of each of the two days were diffused over the twenty-four hours, their respective temperatures would be constantly 12° and 14°. Hence the subjects of comparison are only *the aggregates of temperature.*

Let us take as a third illustration the quantities of urine given in two series of researches published by Boecker.[1] He gave to an individual, whose diet was in other respects exactly estimated, a certain quantity of decoction of Sarsaparilla daily, and found that the quantities of urine passed were in cubic centimètres as follows:

1467.	1744.	1665.	1220.	1161.	1369.
1675.	2129.	887.	1643.	934.	2093.

During a second series of twelve days, the individual experimented on took, with the same dietary, instead of the decoction, an equal quantity of distilled water, when the quantities of urine were as follows:

1263.	1740.	1538.	1526.	1387.	1422.
1754.	1320.	1809.	2139.	1574.	1114.

It must be remarked that it is assumed that these numbers accurately represent the quantities of urine excreted, so that the Means of the two series may be taken as representing simple averages. Are we, now, to conclude from these numbers that the Sarsaparilla has di-

[1] See Reil's 'Journal für Pharmacodynamik und Toxicologie,' vol. ii, parts 1 and 2, "On the Action of Sarsaparilla."

minished the excretion of urine? Obviously not; for it will be readily seen that if we were to stop at the eighth day in both series, the Means would then be respectively 1554 and 1494, and we might thence, with equal justice, conclude that the urine had been increased by the Sarsaparilla. The difficulty involved by so doing is not removed by assuming that the data of twelve observations are more certain than those of eight, for it might easily happen that the thirteenth altered the relations of the two once more. In fact, if the thirteenth day in both series were to exhibit the same quantities as the twelfth—which would be certainly not at all impossible—the Means would then be respectively 1545 and 1515, and would consequently once more indicate an augmentation of the urine. Where, then, are we to seek for the error of such an inference?

If the quantity of urine excreted depended solely upon the nutritive or medicinal agents ingested, the series of observations would, on the supposition that the ingested matters were daily equal, necessarily give numbers which would be uniform, with the exception of unimportant differences that might be attributed to imperfections of measurement. Practically, however, this is never the case, even in investigations where the greatest trouble has been taken to ensure equality in the measurements;[1] and so far as the case under consideration is concerned the irregular and excessive inequalities in the numbers obtained stand in no sort of relation to those of the quantities of food consumed. Other influences, therefore, must operate upon the amount of the urinary excretion. Amongst those which affect the metamorphosis of tissue, we may enumerate, with more or less probability, atmospheric agencies (the temperature, moisture, and pressure of the air); bodily or mental occupations; idiosyncracies; the frequent and sometimes rapid fluctuations in the weight of the body, &c.

Consequently, when the first eight numbers of series *a* give a greater Mean, and the whole twelve numbers a smaller one, than the corresponding number of series *b*, the inference obviously is, that the sum of all the influences which affected the metamorphosis of tissue during the first eight days of the series *a* (amongst which, in addition to those referred to above, there were probably many others, and possibly that of the Sarsaparilla), *exceeded* the sum of

[1] See amongst other papers, that of Boecker, entitled "Investigations on the Action of Water," 'Nova Acta Academ. Cæs. Leopold. Carol. Nat. Cur.,' vol. xxiv, part 1.

those which affected it in the corresponding eight days of the series *b*. On the other hand, the sum of the influences which operated during the whole of the twelve days in series *a* was *less* than that of those acting during the corresponding days of series *b*. It is possible that the Sarsaparilla may have exercised some influence amongst the other agencies at work; but whether it really did, or not, and whether in the way of augmentation, or diminution, the numbers give us no means of judging. All that they permit us to recognise is that its influence, if present at all, was greatly exceeded and concealed by that of agencies which were extrinsic to the dietary. So that, if we want to draw a conclusion of a *medicinal* character from the numbers in question, the way in which it should be expressed is somewhat to the following effect:—Sarsaparilla may, or may not, affect the quantity of the urine; but so far as any *medicinal* value it may have is concerned, it cannot be from its influence on the urine, since that is so much inferior to the influence of other agencies to which we are every day exposed. This, in fact, agrees pretty well with the interpretation which Boecker himself has put upon his investigations.

Section IV.—*The Arithmetic Mean as the probable value of a definite, fixed quantity.*

When we wish to find either directly or indirectly, by measurements, a *single* quantity of *definite* value, and the different measurements which have been taken by the same or by different methods give, in consequence of the imperfection of the instruments or methods employed, different results, the arithmetic mean value of the numbers obtained is, under the circumstances, the probable value of the quantity of which we are in search; or, in other words, it (the Arithmetic Mean) has, under the circumstances, a greater claim to be regarded as the *real* value than any of the numbers obtained has.

The proof of this proposition is based upon the generally received law, that in the frequent occurrence of any two conditions—with reference to which there is no reason why the one should occur more frequently than the other—the frequency of occurrence of the one becomes more and more equal to that of the other in proportion as the number of instances to be compared increases.

If we take now, a series of measurements of any given quantity, we shall find in reference to it, that, in consequence of the greater or less imperfection of all our means of measurement, not one of the results obtained can be looked upon as absolutely certain, but that each of them is vitiated by a certain degree of error, the *real value* and *amount* of which it is impossible for us accurately to learn. If we have no reason for supposing that the error in each of our measurements is more likely to lie on one side of the normal than on the other, we shall be justified, in conformity with the above law, in concluding—provided the measurements are sufficiently numerous—that the sum of those on the one side is equal to that of those on the other side, i. e., that the Arithmetic Mean of the results represents, as accurately as possible, the measurement required. The conjectural reliability of the mean value depends upon two conditions: (1) that in each of the different measurements the error in one direction does not probably exceed that in the other; and (2) that the number of measurements is itself sufficiently great. Practically in all such measurements of fixed quantities, in which we require extreme accuracy, we never neglect to avail ourselves of the aid which this process offers.

The more recent delicate instruments for angular measurements have, indeed, reached so high a degree of perfection, and their employment is so simple, that the errors arising from the manipulation of them, especially with skilful observers, can be but of very slight consequence; and yet in many cases, e. g., in geodetic operations, the frequent repetition of measurement of individual angles is never neglected, when these measurements are intended to serve as the basis for the computation of numerous other quantities, or for purposes of minute comparison; so that we may obtain an average whose accuracy shall sensibly surpass the limits of that of any individual measurement, or even of that of its smallest graduation.

Much less complete, in comparison with these, are the measurements of quantitative chemical analysis. For the quantitative determination of any one element of a substance, in addition to repeated weighings, numerous operations are often necessary, from none of which can special and unavoidable errors be excluded; so that the inaccuracy of a single determination, which is made up of the sum of various individual errors, sometimes reaches a high percentage. In comparing the chemical composition of two different substances, therefore, it is requisite that our measurements should be repeated in

proportion as the difference in composition of the substances in question approaches the limits of possible error.[1]

A third example of the case in which the Arithmetic Mean represents the probable value of a definite fixed quantity would be found in the two series of urinary quantities given in Section III, as well as in the serial determinations of the principal elements of the urine in the same investigations, supposing (which, in reality, is not the case) the amount of urine excreted, or its chemical constitution were day by day uniformly the same in each respective series. The fact is, however, that these numbers belong to the case which we shall hereafter investigate, where the combined numbers represent the measure of a *changeable* quantity.

In order to characteristically differentiate the Mean described in the previous from that of the present paragraph, it may be said that in the former case we have to do with *several* numbers of *definite* value, whilst in the present one we have to determine a *single* number from several which are of *indefinite* value.

Section V.—*Estimation of the accuracy of the Mean as indicating the probable value of a fixed quantity.*

If the two conditions indicated in the preceding paragraph, under which the Arithmetic Means exhibit the probable value of the quantities to be determined by them, were always fulfilled, the numbers they give should then be treated as absolutely accurate. But as this is never completely the case, a standard is requisite, especially when we compare several Means with one another, by reference to which their reliability may be estimated. I will, therefore, now in-

[1] This will be easily illustrated by an example. Suppose that we are comparing the percentage of carbon, say, in two given substances, and that a certain number of observations gives us 50 as the Mean in one of them, with a fluctuation of 5 (*i. e.*, to 45 or to 55), and, consequently, a *possible range of error* of 10; whilst the other gives us 60 as the Mean, with the same amount of fluctuation (*i. e.*, to 55 or to 65), and the same range of *possible error*. It will be evident that the difference between these two substances, *i. e.*, 10, does not exceed the limit of possible error, which is also 10; hence the necessity either of repeating our observations until the range of possible error is reduced to a much smaller amount, or else of placing little or no reliance upon the difference of percentage composition which the numbers 50 60 would otherwise represent.—*Translator.*

dicate some of the methods by which knowledge on this point may be acquired.

(*a*) *Method of Successive Means.*—An indefinite number of determinations of each quantity to be estimated is made, each of them undertaken with equal care, and under the pre-supposition that the errors in them are as nearly equal on either side of the normal as is possible. With the view of giving increased accuracy to our results, we carry out each figure obtained to three places of decimals. We then take the Means successively of the first two numbers, then of the first three, first four, and so on; in each case to about four places of decimals, when we shall find that the Means obtained will agree with one another at the outset in their whole numbers, but that they differ in their first decimals. In conformity with the principle laid down in the previous paragraph, viz. that the accuracy of the Mean increases *pari passu* with the number of the concurring figures, it will be found that the Means exhibit, after a certain point, a uniformity in their *first* series of decimal places, then in their *first two* places, still later in their *first three,* and at length in *all four* decimal figures; the last of which results, however, is only attained by extending the process to many hundred figures. As soon as we reach this point, we have good ground for concluding that all four decimals are absolutely accurate. By this means we may learn after how many determinations we may depend upon the certainty of the first and each succeeding decimal figure. This, at least, is clear, that in general, if the number of measurements given is a small one, we can always estimate, by employing the process of successive Means, to what length the certainty of the general Mean extends. Of course the number of the later Means, whose concordance with one another is most marked, must be sufficiently large to enable us to feel convinced that their agreement is not merely accidental.

As an illustration of the process, I will take a series of numbers from a paper by W. Kaupp, "On the Dependence of the Amount of Chloride of Sodium in the Urine upon that in the Food." [1]

The series is as follows:

24.300.	24.173.	23.340.	23.600.	26.057.	23.101.
24.511.	23.343.	26.400.	22.650.	23.590.	23.644.

Strictly speaking, however, this series does not belong to the

[1] 'Archiv für Physiologische Heilkunde,' 1856, p. 393.

category now under consideration, since it gives us the amount of chloride of sodium determined in the urine during twelve consecutive days, and consequently not the measurement of a single and invariable quantity; but, as all we want is an illustration for the purpose of demonstration, we will, in using it, assume that the composition of the urine remained unaltered throughout these twelve days.

By taking the successive Means, we get

As the Mean for the first	2 days	24.2365
" " " "	3 "	23.9377
" " " "	4 "	23.8533
" " " "	5 "	24.2940
" " " "	6 "	24.0950
" " " "	7 "	24.1544
" " " "	8 "	24.0530
" " " "	9 "	24.3138
" " " "	10 "	24.1474
" " " "	11 "	24.0967
" " " "	12 "	24.0590

It will be observed that all the Means, from the third downwards, agree in their first two figures, and we are, therefore, justified in regarding them (i. e., the figures 24) as certain. It is not even necessary for this purpose to exclude the first three Means, since the second differs by less than one unit, and the third by less than two units of decimals from 24. In the third figure, however (i. e., the first decimal), there is no evident approach to uniformity; yet we see that all the numbers differ from 24 by less than .4, and we may therefore conclude that the real value lies somewhere between 24 + .4, and 24 — .4, i. e., if we take 24 as the round number, the limit of uncertainty will be .4. It will be still more desirable to take the Arithmetic Mean of the first decimals as the basis for our general Mean, so that in the fluctuations of the first decimals no preference may be given to any one of the successive Means above the others. The successive Means, leaving out all but the first decimals, are— 24.2. 23.9. 23.9. 24.3. 24.1. 24.2. 24.1. 24.3. 24.1. 24.1. 24.1; consequently the Arithmetic Mean of the series = 24.1, and the greatest divergence from 24.1 is only .3, since all the successive Means lie between 23.85 and 24.32; so that for all practical purposes we may assume 24.1 as the value of the Mean, with an uncertainty = .3.

In so doing, however, we presume that by a further continuance of the observations no greater divergence is exhibited by the successive Means than has hitherto occurred, which, when we consider the small fluctuations in the decimals of the above eleven figures, is sufficiently improbable to justify our taking .3 as the maximum of uncertainty.

The rule to be observed in such cases as this is as follows:

The *mean number* of which we are in search is to be extracted from the figures which a reference to the Successive Means indicates as *certain;* we then take as our next figure the Arithmetic Mean of the first series of uncertain figures, employing as many of the last successive means for this purpose as the nature of the case may lead us to consider advisable.

The *measure of uncertainty* will then consist in the greatest divergence exhibited by the figures of the series from the mean number obtained as above.

The method just indicated for forming an estimate of the accuracy of the Mean, and which I have denominated "the Method of Successive Means," has not, so far as I am aware, been anywhere previously exhibited or applied; yet it offers, provided the numbers employed are sufficiently numerous, in all cases *the most exact result.* It will be seen in the sequel of how many applications, to which we could not by any other expedient have recourse, the process of successive means is capable.

(*b*) *Estimation of the accuracy of observations by the aid of the "Mean Error."*—It has been before remarked that the presumption is, that every observation is tainted by a certain error. The extent of this error, and consequently the real value of the quantity determined, will always remain unknown; and if we take this error into estimation according to the rule of probabilities, what we must consider it to represent is, not the divergence of the observations from the *real,* but only from the *probable* value sought for—in our case, that is, from the Arithmetic Mean. This error is stated as positive or negative, according as the value of the observation in question is greater or less than that of the general Mean. It is obvious that the smaller these errors are, the better will the observations be; and hence we are justified in estimating the value of a series of observations by the magnitude of its errors. To take the greatest error as our standard would be too one-sided; just as it would be to confine ourselves to taking only the largest and the

smallest observations as the basis for estimating the measure of error; since it might easily happen that, with a number of small errors, one or at most a few large ones might be present, in which case we should get too unfavorable a standard. In forming our estimate, therefore, we take into consideration *all the errors together*, and assume as our standard the mean of the whole of them. This mean we call the *mean error*. The only question to be decided with regard to it is, which kind of Mean is best adapted for the purpose? The Arithmetic Mean is obviously unsuitable, since we have seen in Section II (*a*) that the sum of the differences of the numbers of a series from their Arithmetic Mean (or the sum of their errors) always $= 0$; the Arithmetic Mean of the differences, therefore (which is, in fact, the mean error), would always be *nil*. We might, certainly, obviate this difficulty by taking all the errors together as positive (in which case, in the example given in Section II (*a*) of the series 3, 6, 9, 4, 13, the mean error would be $\frac{4+1+2+3+6=16)}{5}\frac{16)}{5}$; but it is considered preferable to employ the Quadratic Mean according to the process adopted by Gauss and Legendre.

I cannot, in this case, allow myself to enter into a theoretical explanation of the reasons for adopting this method of determining the mean error, firstly, because it would lead me too far away from the subject in hand, and, secondly, because it would presuppose, as requisite for its comprehension, an amount of mathematical knowledge which, in the circle of readers for whom this treatise is designed, is not to be expected. I will not, however, omit to mention two reasons which will be intelligible without any deep acquaintance with mathematics. The first of these is, that squares [i. e., squared numbers] are always positive; and, consequently, both positive and negative errors exercise equal weight in determining the mean error. The second reason depends upon the peculiarity of the Quadratic Means referred to in Section II (*d*), viz., that they increase, where the Arithmetic Means are identical, with the inequality of the observations. Hence two series of observations, whose errors, considered absolutely,[1] give the same Arithmetic Mean, but of which one contains more unequal and, therefore, more important errors than the other, are obviously of unequal value. The series in which the errors are the more un-

[1] The more accurate form of Arithmetic Mean, in which the errors occur with their signs, is not, naturally, in question here, since it would always give 0 as its mean error.

equal offers, without question, the less certainty of the two, and must therefore be considered as the least accurate—which fact is expressed by the greater amount of its Quadratic Mean error, whilst, so far as the Arithmetic Mean error is concerned, the two series would appear to be equally accurate.

Still less applicable than the Arithmetic Mean are the Geometric and Harmonic Means, since instead of exhibiting a greater mean error for the more unequal series, they would exhibit a smaller one. For the sake of comparison, we may here take the mean error of Kaupp's series as given above. The errors, i. e. the differences of the observations from the Arithmetic Mean 24.059, are serially—

— .241.	— 1.998.	— 2.341.
— .114.	+ .958.	+ 1.409.
+ .719.	— .452.	+ .469.
+ .439.	+ .716.	+ .415.

The squares of the errors are consequently—

.058.	3.992.	5.480.
.013.	.918.	1.985.
.517.	.204.	.220.
.211.	.513.	.172.

The Arithmetic Mean of these squares is 1.190, and consequently the mean error $= \sqrt{1.190} = 1.091$.

We, therefore, judge of the value of a series of observations by the magnitude of its mean error, i. e. we say that one series is so many times better than a second series, in proportion as its mean error is less than that of the first; for instance, a series, whose mean error is 6, is six times worse than another whose mean error is 1; or, in other words, the value of the first series is one sixth of that of the 2d. In the same way the value of a third series, whose mean error is 4.3, would be only $\frac{1}{4 \cdot 3}$ in comparison with that of the second of the preceding series. Hence, if we take the value of a series, with 1 as its mean error, at unity, the value of any other series with the mean error n will be expressed by its reciprocal value, i. e. by $\frac{1}{n}$. So that the value of Kaupp's series $= \frac{1}{1 \cdot 091} = .917$.

I may remark, for those readers who are acquainted with the princi-

ples of the calculus of probabilities, that I have here intentionally laid myself open to the charge of inaccuracy. In reality, what is generally taken as the measure of value is not the reciprocal value $\frac{1}{n}$, but its square $\frac{1}{n^2}$, for which there are good reasons. These reasons, however, are only of importance when we wish to combine, for the purpose of more extended calculations, series of observations of unequal value, which refer to the same quantities. For the object which we have in view, which does not require an application of that kind, the simpler and more easily comprehended process for estimating the measure of accuracy is equally efficient.

(*c*) *The Probable Error.*—If the probable value of the quantity to be found from a series of observations (which here, too, is the Arithmetic Mean) $= a$, we may take another fixed number b, of such a nature that, if the observations were repeated very frequently, their results would lie as often within the limits of $a - b$, and $a + b$, as without them. And if β were ever so little greater than b, it would be more probable still that the next observation would rather fall within the limits of $a - \beta$ and $a + \beta$, than without them. The number b is called the *Probable Error,* and it is obtained, in accordance with the rules of the calculus of probabilities, by multiplying the mean error by the numbers .67449, i. e. by nearly two thirds of the mean error.

In Kaupp's series, quoted above, the probable error will, therefore, be $1.091 \times .674 = .736$. That is to say, if the observations in question were repeated sufficiently often, their divergence from the mean 24.059 would more, rather than less, frequently be within the number .736; or, in other words, would more often be within the limits of 23.323 and 24.795, than without them.

It must, however, never be forgotten that, in laying down these rules, it is always presumed that the Arithmetic Mean really represents the probable value of the quantity under observation. If this should happen not to be completely the case, i. e. should the observations not be sufficiently numerous, the product, which we have called the "probable error," will, in a corresponding degree, possess more or less of the characteristic referred to; *but when the series of observations is too short, it is no longer applicable.*

SECTION VI.—*The Arithmetic Mean as the probable value of a variable quantity determined under mean conditions.*

With the view of facilitating the reader's comprehension of this condition, by placing a definite example at once before him, let us take as the basis of our description the case with regard to which we are in the present instance more particularly interested, viz. where the mean is taken from a daily series of observations of the amount of urine excreted by an individual; or, of the quantities of different substances which the urine may contain. In this case, the observed quantity varies from day to day, in consequence of the different external influences which cannot be voluntarily regulated, as well as of the variable physical condition of the individual himself. If both of these causes were to produce no fluctuation, the quantity which the different observations would give would be invariable, and we should then have the case of Section IV. If, on the other hand, the observations were entirely free from error, whilst the fluctuations in question had full play, we should then have the case referred to in Section III. The Arithmetic Mean would in this case be a simple average, and would accurately represent the value which the quantity under observation (if the observations were absolutely accurate) would have, if the individual from whom it was taken were placed in a constant mean physical condition, and if the variable influences above referred to were to spread their action uniformly over the whole space of time during which the observations were made. This quantity, which would be entirely definite, might be called the *mean quantity.* On account, however, of the errors to which all observations are liable, the Arithmetic Mean which we practically obtain will in reality only represent, in conformity with Section IV, the probable value of this mean quantity (or average). In order, however, that it may in fact represent the probable value, the conditions indicated in Section IV must be as nearly as possible fulfilled, i. e. the individual quantities of each day must be obtained as the Means of observations repeated as often as is practicable—indeed, of observations that are repeated with a frequency proportionate to the inaccuracy of the methods of measurement. The chemical analyses especially should be performed over and over again. In the determination of the urinary quantities, to be sure, a repetition of the measurements is as inapplicable (unless, in addition to the volumetrical

determinations, the check of determinations by weight be employed, as was done by Boecker) as unnecessary, since the errors of measurement, when contrasted with the other irregularities, are altogether unimportant. Those which arise from the greater or less completeness with which the bladder may be emptied must be placed to the score of varying influences, and of the accompanying physical conditions on each occasion, so that their equalization may find account in the eventual increase of the day's observation.

Section VII.—*Comparison of the Means of several complete series of observations of variable quantities.*

Let us suppose that two series of observations, of a nature analogous to those described in the previous paragraph, are instituted on the same individual, during one of which he is exposed to a given agency, whose influence on the excretions we wish to ascertain. The question which we have to determine is if, and to what extent, the Arithmetic Means of the two series, which, in accordance with the previous paragraph, exhibit an approximate value to the averages, can give any conclusion on the point. With the view of answering this question, I shall proceed to lay down certain fundamental principles in the present paragraph.

We will call the series in which the supplementary agency has operated A, and the other series B; the Arithmetic Mean of the first series m; that of the second, n; and indicate the limits of accuracy of the individual observations by a. If the observations had been completely free from error, we should have had instead of m another number, which I will call P; and in the place of n another number, p. P and p are, then, what has been above called *mean quantities*. Suppose, for instance, that the question were a comparison of quantities of urea, and that series A gave 32.4 as a mean, and series B 30.6, and also that in the determination of urea the greatest error that should occur is .6; then would $m = 32.4$, $n = 30.6$, and $a = .6$. The value of P would therefore lie between $m—a$ and $m + a$, i. e. between 31.8 and 33; the value of p between $n—a$ and $n + a$, i. e. between 30 and 31.2, since the limits of uncertainty are the same for the Arithmetic Mean itself as for each of the individual observations. As a is the greatest supposable error of any one observation, the most unfavorable

state of things that could occur, though, nevertheless, a possible one, would be that each observation should be either too large or too small by a. If, therefore, a series contained, say 10 observations, their sum would be either too large or too small by $10\,a$; and in like manner the Arithmetic Mean would be in error by $\frac{10\,a}{10}$, i. e. by a.

Further, if the influences operating during the periods when the serial observations A and B were made were of such a nature that their daily average effect, supposing that the supplementary agency were not to interfere, were for series A always exactly the same, then the difference between P and p, viz., $P - p$, would express rigorously the daily average influence, and we should infer that its effect was of an augmenting or diminishing nature in proportion as we found $P > p$ or $p > P$.

But P and p are unknown, all that we know being that P lies between $m - a$ and $m + a$, and p between $n - a$ and $n + a$. If, therefore, we wish to be certain that there has been, for instance, a real increase in the excretions, i. e. that $P > p$, we must have $m - a > n + a$, since it is possible that $P = m - a$ and $p = n + a$. Hence, what we must compare are not the Means m and n which we obtain from the two series of observations, but the numbers $m - a$ and $n + a$,—i. e. in the above illustration, 31.8 and 31.2.

Since it follows obviously from $m - a > n + a$ that $m - n > 2\,a$, we may avail ourselves of the following expression:

In order to allow us, in the case under consideration, to come to the conclusion that the excretions have been increased, the difference between the Means (viz. $m - n$) must be greater than the double of the largest possible error of observation, i. e. GREATER THAN THE SUM OF THE UNCERTAINTIES *of these Means.*

In the same manner, if we wish to be certain of a diminution having taken place, i. e. that $p > P$, we must find that $n - m > 2\,a$, since the difference between p and P may be less by $2\,a$ than that between n and m (i. e. since we may have $p = n - a$ and $P = m + a$).

The fulfilment of the above-mentioned condition, viz., that after eliminating the operation of the supplementary agency the average excretion in series A should equal the average in series B, can never be calculated on in practice; hence it is necessary thoroughly to investigate the circumstances in which it is not fulfilled.

With the view of demonstrating the point in question more precisely, I shall make some preliminary observations.

The numerically expressed increase or diminution of the excretion under investigation, which is the result of some definite cause, may obviously be regarded as the gauge of the influence which this cause exerts, and may be expressed by the phrase "amount of the influence," or more succinctly by the word "influence;" and this number may be assumed as positive or negative, according as it refers to an increase or a diminution of the excretion. Thus, if, for instance, the series A embraces eight days, and the series B twelve days, the sum of all the influences at work in series A will equal 8P, and those in series B will equal $12p$. Let us further assume that the number P consists of two elements, p' and q; of which q expresses the effect of the supplementary agency, and p' the average value of all the other influences: the *sum* of these latter will in series $A = 8p'$. In the suppositions case referred to above, where, after the elimination of the supplementary agency, the average quantity of the excretions in both cases was equal, and consequently $p' = p$, the sums of the influences were $8p$ and $12p$, and their relation to one another, therefore, was 8 : 12. Hence we may characterise this case by the expression, that in it the sums of the influences in the two series bear to one another the proportion of the numbers of the days during which the observations are carried on in the series respectively.

We will now dismiss from consideration the preceding limited contingency, and direct our attention to the more probable case where p and p' are unequal, temporarily assuming that the physical condition of the individual remains unaltered, i. e. that any one cause always exerts a uniform influence upon the excretions. Thus, for example, the daily influence of the supplementary agency, supposing its daily amount to be uniform, will experience no fluctuations and will exactly equal its average q. Further, the variations in the excretions, whose averages are p' and q, will depend entirely upon the magnitude of the influences that operate upon them from without (upon the nature and quantity of the nutritive material, bodily activity, &c.)

It must also be understood at the outset that those influences which, as in the case of atmospherical ones, are dependent upon the season of the year and the state of the weather, either do not operate at all, or if they do, do so uniformly. Under these circumstances it can be predicted with great probability that under the

observance of as uniform a mode of life as possible the remaining influences will fluctuate as little above as below a certain mean, so that we may apply the law laid down in Section IV, viz., that with a sufficient number of observations the Arithmetic Means of the influences become more and more identical. (This approach to uniformity will also make itself apparent in the Arithmetic Mean of the observations, since these only differ from the real amount of the influences at work by the errors of observation.) If, therefore, these real amounts were known, and if one were to take their excessive means, their fluctuations would gradually but manifestly diminish and converge to a certain Mean value (which we will call p_0). If we now represent the divergence of the quantities p' and p from p_0 by β' and β respectively (consequently $p' = p_0 \pm \beta'$,[1] and $p = p_0 \pm \beta$), β' and β will gradually become less and less, and p' and p more and more uniform, as soon as the observations are sufficiently numerous. The daily average quantities of the series A and B, viz., $p' + q$ and p, which are assumed to be free from errors of observation, will also approach nearer and nearer to the fixed values $p_0 + q$ and p_0.

Let us take, again, as an example, Kaupp's series of observations quoted in Section V, *a*, which is more suitable here than it was there, where, in order to be able to employ them, we were obliged to assume the fiction that the quality of the urine did not change during the twelve days. For our present purpose the only fiction which it is necessary for us to introduce is that physical conditions and atmospheric influences remain uniform during the period of observation. In this instance q indicates the daily influence of the dose of chloride of sodium, and if we assume at the outset that the analyses are sufficiently accurate to limit the influence of the errors of measurement to the third decimal, we may conclude that first two decimals are free from errors of observation. Of the successive Means (commencing with the sixth) 24.10 — 24.15 — 24.05 — 24.31 — 24.15 — 24.10 — 24.10, $p' + q$ represents the value of the first (supposing that the observations were broken off on the sixth day), $p' + q$ represents that of the second (if they were broken off on the seventh day), and so on, until the twelfth, of which the value would also be $p' + q$. But, as q would be constant, under the supposition that the concomitant physical conditions would be unaltered, the fluctuations

[1] The double sign $\pm$ is used because the real quantity p' may either exceed the mean value by β', or be less than it by the same amount.

would depend entirely on the variability of p'. We cannot in this case perceive any contraction of the limits within which the numbers fluctuate, since the number of the observations themselves is comparatively so small. The extremes between which the seven numbers lie are 24.05 and 24.31; their Arithmetic Mean is 24.14; the divergencies of the latter from the seven numbers represent the fluctuations of the successive Means about the mean number 24.14; and, consequently, the greatest fluctuation is $24.31 - 24.14 = .17$. If, as stands to reason, on a more extended continuation of the investigation, the fluctuations of the additional numbers did not exceed this amount, we should assume this mean 24.14 as the approximate value of $p_0 + q$, i. e. the value to which the Arithmetic Mean of the numbers, when freed from errors of observation, would approximate if the observations were repeated indefinitely. The quantity β' above mentioned indicates the divergence of this maximum of fluctuation $(p_0 + q)$, which is only to be obtained, by indefinitely repeated observations, from the Arithmetic Mean of the actual observations (freed from errors); hence, in the present case, if $p_0 + q$ were *exactly* $= 24.14$, β' would $= 24.14 - 24.10 = .04$, for the twelve days' observations, since 24.10 was their mean.

On the supposition that we have reason to conclude that, by continuing the observations, the fluctuations would never transcend .17, we are justified in assuming that number as the limit of uncertainty of the Mean 24.14, and (as the number 24.14 previously assumed for $p_0 + q$ was not the true but only an approximate value) that the real value of $p_0 + q = 24.14 \pm .17$; i. e. in supposing that $p_0 + q$ lies somewhere between 24.14—17 (or 23.97) and 24.14+.17 (or 24.31).

If, now, we place by the side of the series referred to, A, a normal series, B, which may in the same manner have given as its result $22.38 \pm .21$, and, therefore, $\beta'_1 = .21$, and $p_0 = 22.38 \pm .21$, we should be obliged to subtract the number $23.38 \pm .21$ from $24.14 \pm .17$ (since we obtain the effect of the dose of chloride of sodium ingested, viz., q, by subtracting p_0 from $p_0 + q$; and here $p + q = 24.14 \pm .17$, $p = 22.38 \pm .21$).

A positive result would indicate an increase; a negative, a decrease; a neutral one, no effect at all. But since the numbers to be subtracted have a certain amount of inexactitude, and we only know that one lies between $24.14 - .17$ and $24.14 + .17$, and the other between $23.38 - .21$ and $23.38 + .21$, an increase, for instance, is only

certain on the condition that the smallest value which the former can possibly have is greater than the greatest value which the second can possibly have, i. e. we must compare 24.14 — .17, or 23.97, with 23.38 + .21, or 23.59; or, in other words, the difference of the Arithmetic Means 24.14 — 23.38 must exceed the sum of the uncertainties of these Means, viz., .17 + .21.

If the measurements were inexact, as in the case above given, and the limit of error of measurement (previously indicated by a) about .2, then the sum of the uncertainties of the agency under estimation would be increased by the sum of the uncertainties of the measurements, viz., $2a$; i. e. in order to justify a conclusion of increase of excretion, it would be necessary that the difference of the mean numbers 24.14 and 23.38, extracted, as directed, from the successive Means, should exceed the sum of all the uncertainties, i. e. $\beta' + \beta + 2a$ (here .78). Where the observations are sufficiently numerous, the special increase necessitated by the errors of observation is not required. But since in practice the observations of which we avail ourselves are not free from error, and we are obliged to undertake the supposed operations with the uncorrected figures, the resulting uncertainties (indicated by β' and β) will not only include within themselves those dependent on the errors of observation, but also those caused by the inequality of the daily influences, so that the actual $\beta + \beta'$ contains, in addition to the theoretical $\beta + \beta'$, the necessary increase.

Moreover, with regard to atmospheric influences, these will also partially establish an equilibrium amongst themselves in extended periods of time, though they will remain in some respects irregular; the result of which will be that the quantities above indicated by p and p' will not approximate any further to a *constant* limit p_0 with the protraction of the investigation. Therefore, where the differences between the two mean quantities to be compared are slight, the certainty of the process of comparison above described will be more or less diminished in proportion as we have reason to attribute greater or less influence to the atmospheric influences. It would only be possible to make a correction for these disturbances in the process, if we could, by means of specially instituted researches, acquire a more exact knowledge of the influences themselves. Where the difference between the mean quantities is considerable, the correction is superfluous; and where it is small, we must be content with previously laying the periods of observation of A and B

by the side of one another, and so making our selection that the atmospheric variations may compensate each other as much as possible. Investigations have, indeed, been made with the view of estimating the influence of atmospheric agencies, but they only extend to those of temperature, and with reference even to this point are very deficient.

We have still to discuss the changeability of the physical condition of the individual, which must have the same effect in altering the above supposed constant p_0 as atmospheric influences. That alterations in the physical condition of an individual do occur there can be no doubt. Indeed, physiologists assume a continuous change in the human economy as a result of the metamorphosis of tissue. A man to-day is not in all respects exactly what he was yesterday; but it is open to doubt whether sufficiently important changes occur within the comparatively short period of time embracing these two mutually related series of observations to bring into operation agencies which perfectly neutralize the above-mentioned influences; in that case special causes must be at work. Such a cause may be—the change in the mode of life which is necessitated by the investigation itself, which must exhibit a continuous operation in the same direction, and, therefore, in an increasing degree, as well as a continuous change in the normal state of health of the individual. The usual daily alternations in the normal state of health are to be attributed to the fluctuating operation of the above-mentioned influences, and have a natural tendency to compensate one another. In order to ascertain whether a recognisable alteration has taken place in the physical condition of the individual during the period of investigation, the method instituted by Boecker of a normal investigation is advisable, i. e. to institute a series of observations without the additional agency, some before, and some after the other inquiries, the periods of which inquiries must be of a much longer duration than that of the other. If we could exactly estimate the physical conditions (or susceptibility to external influences) on every occasion, we might then multiply every observation by the coefficient determined for its physical condition, so as to reduce all our observations to a uniform standard. But, as this is impossible, we must consider, in the case of minute differences between the mean quantities, those series of inquiries to be inadmissible in which excessive changes in the physical condition have exhibited themselves; where, however,

this is not the case, the process above mentioned may be considered as accurate.[1]

In repeating the outline of this process I must also repeat the observation, that it is only available for *complete series*, by which I mean those series whose duration is sufficiently long to allow of our recognising, by the nature of the Successive Means, whether the irregularities of the observations have reached a notable degree of compensation or not.

Let M and N be the Means of the series treated, with their disturbances, but only proceeding so far as the numbers which the successive mean determinations have demonstrated to be concordant. The last of these concordant figures must then be treated after the method of abbreviated decimal fractions (i. e. be raised by one unit if the succeeding figure be 5 or greater than 5), and where it is greater than unity the places following it are to be filled up by ciphers. For instance, if the Means were 6842 or 2693, and their certainty were limited to two figures, they would be respectively noted as 6800 and 2700. Where the fluctuations of the first uncertain figures in a sufficiently large combination of the last successive Means is slight, we may (as in the case of the above example) append the Arithmetic Mean of these first uncertain figures to the certain ones. We then compare the difference M—N, or N—M, with the sum of the uncertainties (the above $\beta' + \beta + 2\alpha$), i. e. with the sum of the greatest divergencies of the group of successive Means from M and N. We shall then be justified in inferring an increase, or diminution, so soon as M—N or N—M exceeds that sum. If neither the one nor the other is the case, the observations leave the operation of the agency experimented on undetermined.

This process, which is based upon the method of Successive Means, so far as perfect series are concerned, leaves nothing to be desired in point of accuracy. Another process, which is founded on the employment of the Mean Error, or rather an analogue of the Mean Error, is very inferior to it, first, because it demands that the differences between the two Means to be compared shall be great, in order to give a reliable determination, and, secondly, because the Mean Error does not directly express the limits of uncertainty, but only the value of the observations according to a conventional standard. But as the process is to be pre-

[1] When the differences between the mean quantities are great, the variations in the physical condition are of no importance.

ferred in the case of *incomplete series*, I shall here describe it, only presuming that the series to which we are applying it are complete. But it must be noted that the justification of this presumption, as a rule, is only to be obtained by proving the Successive Means.

Let M' and N' be the Arithmetic Means of A and B; in obtaining which we take not only the *certain* figures, as before, but two or three of the uncertain ones. We then evolve the Quadratic Means from the divergencies of the observations of the series A and B from M' and N' respectively, i. e. from the fluctuations of the observations about these two mean numbers. If the fluctuations in the observations arose from errors, then, in accordance with the definition in Section V, *b*, the Quadratic Means would represent the mean errors of A and B. But as they depend, for the most part, upon the inequality of daily influences, I shall call the Quadratic Means, which we will suppose to be respectively μ and ν, not the Mean Errors, but the *mean fluctuations;* and, in like manner, the analogue of the probable error, the *probable fluctuation.* Now, since, according to Section V, *c*, the Mean Errors are greater than the probable ones, and also the mean fluctuations greater than the probable ones, we shall be justified in assuming that, by an increase of the number of observations, the subsequently determined numbers will more frequently fall between $M'-\mu$ and $M'+\mu$ (or $N'-\nu$ and $N'+\nu$) than without those limits. And, as a consequence, we shall consider it more probable that if we protracted the investigation with the view of obtaining more accurate and certain results, the certain Means would lie between $M'-\mu$ and $M'+\mu$ (or $N'-\nu$ and $N'+\nu$) than the reverse. Hence, by proceeding in a similar course to the one observed above, the rule which we should establish is the following: the employment of the supplementary agency would have probably produced an increase (in the excretions) if $M'-N'>\mu+\nu$; a diminution, if $N'-M'>\mu+\nu$; and that its operation was undetermined if the difference between the Means M' and N' did not exceed the sum of the mean fluctuations.

SECTION VIII.—*On incomplete series of Observations.*

The determinations of the previous paragraph require that the series of observations shall be complete, i. e. that they shall extend far enough to allow the inequalities in the Arithmetic Means to compensate one another, which, as we know, may be recognised by

the limits within which the Successive Means fluctuate commencing markedly to approach one another. In practice, however, we shall, as a rule, have to deal with the case where this condition is not fulfilled; hence the question arises, how we ought to deal with incomplete series.

In order to answer this question it will be of use to make some preliminary observations on the course of the Successive Means in cases of this description. For this purpose let us first note that, in general, any disturbing agency does not exert its full influence at once, and also that with its diminution that of its operation does not concomitantly occur. Even in the case where the agency is but of short duration, we should find, if we could isolate its effect, and were to make an hourly examination of the urine, that it exhibited a gradual increase to a maximum, and then an equally gradual diminution. Further, we note that a disturbing agency may either operate all at once or may periodically repeat itself. In practice, we have little room for doubting that periodically acting influences are at work along with regular ones, since we can detect in the vital processes numerous distinct periods. It will, therefore, be desirable to consider both conditions separately, in order the more easily to appreciate their combined operation.

(*a*) We will suppose the agencies at work are all of a periodic character, and, first, that only a single one is present, whose period, for the sake of simplicity, shall embrace a uniformly equal number of entire days, and always have the same amplitude, i. e. exhibit the same series of values. Let the period be, for example, one of four days, expressing itself in the numbers 1, 3, 11, 7, and producing the series 1, 3, 11, 7; 1, 3, 11, 7; 1, 3, 11, 7, &c., whose successive Means are as follows: 1, 2, 5, $5\frac{1}{2}$; $4\frac{3}{5}$, $4\frac{1}{3}$, $5\frac{2}{7}$, $5\frac{1}{2}$; 5, $4\frac{4}{5}$, $5\frac{4}{11}$ $5\frac{1}{2}$; $5\frac{2}{13}$, 5, $5\frac{2}{5}$, $5\frac{1}{2}$, &c. It will be seen that in these Means the fluctuations in each period become gradually smaller and smaller. Notwithstanding the inequality of the given numbers, already, in the second period, the fluctuations do not exceed 1; in the third period they do not exceed $\frac{1}{2}$; and even by the middle of the first period they exhibit a commencing equalisation. If the series does not break off with the end of a period, but in the middle of one, what we shall get is not the exact Mean of the period (which here $= 5\frac{1}{2}$), but a sensible approximation to the true value $5\frac{1}{2}$, in the second period, which increases in correctness with the number of the periods. Con-

sequently, if we were ignorant of the extent of the period, but had reason to believe that the investigation had broken off after the conclusion of the first one, the last Mean would be an approximately correct one, and we might assume as the limit of error the greatest fluctuation which the series of Successive Means exhibited in so many of its latter values as the period possessed conjecturally members. Thus, supposing that the above series broke off at the fourth 3, the Mean would be 5; and if we had reason to think that the period had not more than five members, we should compare the last five Successive Means, viz., the numbers $4\frac{4}{5}$, $5\frac{4}{11}$, $5\frac{1}{2}$, $5\frac{2}{13}$, 5; and since amongst them $5\frac{1}{2}$ is the number which diverges most from 5, we might assume $\frac{1}{2}$ to be the limit of error. But a better course still would be to take as the probable Mean, not 5, but the Mean of the above five Means, or, in round numbers $5\frac{1}{7}$, by which method the limit of error would be reduced to $5\frac{1}{2} - 5\frac{1}{7} = \frac{5}{14}$.

Suppose that we assume now that a second periodically acting influence intervenes, that its time of action is three days, and that its members are 4, 8, 3, so that the series 4, 8, 3; 4, 8, 3, &c.—with the Successive Means 4, 6, 5; $4\frac{3}{4}$, $5\frac{2}{5}$, 5; $4\frac{6}{7}$, $5\frac{1}{4}$, 5; $4\frac{9}{10}$, $5\frac{2}{11}$, 5, is produced. The numbers which are obtained will be the sums of the two periodic series, viz., 5, 11, 14, 11, 9, 6, 15, 15, 4, 7, 19, 10, &c., and the Successive Means will be the sums of the two sets of Successive Means, viz., $1+4$, $2+6$, $5+5$, $5\frac{1}{2}+4\frac{3}{4}$, $4\frac{3}{4}+5\frac{2}{5}$, $4\frac{1}{3}+5$, $5\frac{2}{7}+4\frac{6}{7}$, $5\frac{1}{2}+5\frac{1}{4}$, $5+5$, $4\frac{4}{5}+4\frac{9}{10}$, &c., i. e. 5, 8, 10, $10\frac{1}{4}$, 10, $9\frac{1}{3}$, $10\frac{1}{7}$, $10\frac{3}{4}$, 10, $9\frac{7}{10}$, whilst the true Mean is $5\frac{1}{2}+5=10\frac{1}{2}$.

It will be seen that so soon as we pass beyond the limits of the first period, indeed, even at the commencement of the second half of that period, a sensible approximation to the true Mean begins to be evident. If, now, the series broke off, suppose at the tenth number, we should have as our ultimate Mean $9\frac{7}{10}$, and (again assuming that the period did not exceed five days) we should have as the limit of error $10\frac{3}{4} - 9\frac{7}{10} = 1\frac{1}{20}$; or,—if we take, in order to reduce these limits, the Arithmetic Mean of the last five mean numbers as the probable Mean, in round numbers 10, with $\frac{3}{4}$ as the limit of error.

The reason for the diminution of the fluctuations in the effective mean series lies in the fact that they can never exceed the sums of the fluctuations which the corresponding Successive Means of the two original series exhibit. It will hence be evident that even in case of the concurrent action of two influences, each of a definite

period, in the series of Successive Means, the fluctuations about the Mean will diminish with every step beyond the limits of a single period; and, above all, that a manifest diminution will occur so soon as the limits of the first period shall have been passed, and that, as above, we shall be able to determine more definitely the limits of error.

Even if the periods in the individual concomitant series of fluctuations are not of sufficiently equal length and magnitude, no important alteration will be effected in the above result, except that in the most unfavorable cases, if, at the termination of the investigation, periods of shorter duration should interpose, the limits of error might turn out to be a little too small.

(*b*) Let us suppose that the fluctuations are occasioned by an influence that acts all at once, that is, by a *disturbance* in the more exact sense of the word. Thus, to take an illustration from a definite set of figures, suppose that the disturbance developed the numbers 2, 6, 8, 4, and that we assume that in the absence of this disturbing cause the numbers would exhibit invariably the value of 1. The series, when thus disturbed, would be 3, 7, 9, 5, 1, 1, 1, 1, &c., and the Successive Means will be 3, 5, $6\frac{1}{3}$, 6, 5, $4\frac{1}{3}$, $3\frac{6}{7}$, $3\frac{1}{2}$, $3\frac{1}{4}$, 3, &c. The latter diminish regularly from the number $6\frac{1}{3}$, so as to approximate to the radical value 1; and the further we proceed the more gradual does this approximation become. At the ninth day the Mean is about $3\frac{1}{4}$; at the tenth it has sunk to 3 (and consequently only differs from the radical value by 2); at the twentieth day it has reached 2; at the thirtieth, $\frac{5}{3}$; at the fiftieth, $\frac{7}{5}$; at the one-hundreth, $\frac{6}{5}$, and so on. Hence, the disturbing agency, when of a serious character, will not cease to appear in the Mean till late in its course, unless other causes of disturbance intervene which operate in an opposite direction to it. Whence we learn that if the numbers in a series of Successive Means either increase or decrease very slowly, so as at length almost to remain stationary, this cannot be entirely taken as a proof of a complete equalisation of the disturbing agencies. In the case above mentioned, for instance, the influence of the disturbing agency is still evident in the Mean at the tenth day, although it has lasted for four days.

If it should happen that numerous disturbing causes should come into operation during the period of observation, and if this latter should be short, it may just as easily happen that they operate in one

direction, as in opposition to one another, i. e. they may just as readily cause an increase as a diminution of the general disturbance.

Let us further suppose, as before, that we have to do with a single disturbing agency, but that, instead of operating at the commencement, it acts at the end of the period of observation. For the sake of better comparison, let us again take a period of ten days, with the same disturbance, giving a series of 1, 1, 1, 1, 1, 1, 3, 7, 9, 5. The Successive Means will then be 1, 1, 1, 1, 1, 1, $1\frac{2}{7}$, 2, $2\frac{4}{9}$, 3. As is natural, although in this case the last Mean (that of the tenth day) is the same as in the first case, the series is a much more favorable one for definite application, since the fluctuations between the Successive Means are much smaller, and consequently the approximation of most of the latter ones gives a much more accurate result than in the case where the disturbance lay at the commencement.

(*c*) Let us lastly consider the case with which we shall in practice have mostly to deal, viz., that where, in addition to periodically acting agencies, irregular disturbing causes come into play. It is clear that if the interval of observation exceeds by much one or more of the periods, the portion of the fluctuations corresponding to it will, to a greater or less extent, have exhausted its intensity upon the last of the Successive Means, whilst the part which corresponds to the longer periods, and to the irregular disturbances, may possibly remain partially or entirely uncompensated. The degree of approximation of the Arithmetic Mean to the value which we should expect to be developed by the investigation will, therefore, depend upon the relation of the disturbing causes to one another and to the periods, and we shall only be able to estimate it with the certainty indicated in case *a*, when the shorter periods are sufficiently extensive. We can only positively recognise the existence of a period, if its extent is very considerable and its length is much exceeded by that of the period of observation, by direct comparison of the observations in general. On the other side, an excessively irregular disturbance, which only extends over a small part of the period of observation, may be generally recognised by taking the Successive Means in two ways—first, from the first observation to the last; and secondly, from the last to the first; and by then noticing whether, in either of the two series of numbers thus obtained, the last half exhibits greater fluctuations than the first.

Section IX.—*Revision of the conclusions drawn from the preceding Observations, with Rules for the institution and conduct of new Investigations.*

If any one should wish to revise any of the more recently published series of observations in conformity with the principles laid down in the foregoing considerations, with the view of rectifying the results that have been obtained from them, he must, in the first place, in view of excessive and continuous fluctuations, lay quite on one side every investigation consisting of not more than three or four days, as, for instance, the 'Investigations on the influence of the internal use of various quantities of ordinary Drinking Water, under different circumstances, upon the Metamorphosis of Tissue in the Human Body,' by Dr. Mosler (prize essay), Göttingen, 1857. I would even, as a rule, lay aside investigations of from five to ten days, if the differences between the single numbers exceed 10 per cent. of the Mean, and if the Means of the series to be compared do not differ considerably from one another; and, as the results drawn from them might easily be the product of disturbing agencies which had extended their influence over the whole period of observation, I would under no circumstances admit them as of value, except on the condition that other series of observations of at least equal accuracy confirmed them.

The question how long series of observations should be in order to justify our drawing an independent conclusion from them, cannot at present be well answered; indeed, an absolute answer is impossible, since the degree of uniformity of the manner of life, the concurrence of certain unavoidable causes of disturbance, the physical condition of the individual subjected to examination, and, in an inferior degree, the accuracy of the observations, will cause differences. In fact, there does not exist a single investigation of sufficiently protracted duration to determine even for a single case the point where the compensation of the aggregate fluctuations has reached an evident stage. The more complete series have hitherto seldom embraced more than twelve to fifteen days, and must in every case be regarded still as incomplete.

All that remains now is to demonstrate a process by which incomplete series may be treated, if we wish to guard ourselves as much as possible from falling into error; and with this object in view, I cannot do better than proceed as follows.

Our first object should be to inspect the numbers supplied by our observations, with the view of examining whether any excessive disturbance has come into play which could give us reason to fear an illusory result. If we wish for this purpose to possess a simple, though indirect test, we should, as above (Section VIII, *c*) directed, form a double series of Successive Means, the one of them proceeding from the first downwards to the last observation, and the other in a contrary direction, and observe whether in either of these series the last half exhibits greater fluctuations than the first. It is only when this is not the case that the series are of any further value. But if both of the series to be compared satisfy this condition, they are then to be treated in the outset as complete (i. e. in accordance with Section VII), i. e. we must examine whether the difference of their Means exceeds the sum of their uncertainties, or not. The uncertainty, as extracted from the Successive Means, will naturally be smaller, and therefore more favorable for the determination of positive results, than the standard of uncertainty expressed through the mean fluctuation, for the former is extracted from numbers in which the inequalities have more or less compensated one another, whilst in the determination of the mean fluctuation all the fluctuations of the original observations come into account. If the series of observations are complete, the limits of fluctuation extracted from the Successive Means would suffice, and we should disregard the amount of the mean fluctuation, since in this case we have to deal with the comparison of influences that have disseminated themselves over the whole period of observation, and not with an examination between what limits they would principally operate if the observations were indefinitely protracted. But if the series of observations are incomplete, the fluctuations occurring during short periods will, as must follow from Section VIII, have eliminated themselves to a considerable extent from the Successive Means; but those occurring during long and incomplete periods, or from irregular disturbances, will, in general, not have done so; that is to say, if the greatest portion of the period falls without that during which the examination is carried on, or if the disturbing influences which would act as compensators should first happen to interpose after the conclusion of the investigation, the fluctuations between the successive Means would be too small; we shall, therefore, generally be less liable to error if we take the mean fluctuation as deduced from the unabbreviated oscillations as the limit of certainty. But under certain

circumstances we shall, even in this case, be able to limit still further the range of error. In the succeeding paragraph I shall show that some series of observations proceed in evident and somewhat fixed periods of two days. If this fact should be established as generally correct, we might then take the observations in pairs, and estimate the mean fluctuation from the obviously lesser fluctuations in the Means of the numbers so paired. In reference to this point, the rule to be observed for future investigations would be, on each occasion, to place the observations in a straight and unbroken line, one after another.

What has hitherto been said with reference to the periodic fluctuations dependent on natural causes is only applicable when the days on which the observations are made follow immediately upon one another. If this should not be the case, as might be expected, a reduction of the limits obtained from the mean fluctuation will no more be permissible.

Finally, it should be thoroughly recognised that the treatment of the incomplete series of observations in the same way as complete ones is only a makeshift, and therefore the conclusion, when the mean differences do not considerably exceed the sums of the uncertainties, must be considered only as of imperfect accuracy, until other researches, instituted for the same object, shall have exhibited a conformable and corroborative result. It is unnecessary to mention that corroborative observations should not be instituted under precisely the same circumstances as their predecessors; that rather, on the other hand, the result is the more decisive the more independent it appears to be of the varying conditions which are associated with the experiments.

SECTION X.—*Examination of certain definite series of Observations.*

Proceeding, by way of conclusion, to examine some published series of researches, our object is—1stly, to point out the impropriety of swerving from the exactitude of proceeding recommended above; 2dly, to illustrate the preceding method by actually existing cases; and, 3dly, to draw attention to the course of the fluctuations in carefully undertaken series of observations, so as, if possible, to get some hints for the institution and utilization of subsequent researches.

(*a*) *Beneke's researches on the influence of Sea Air and Sea Water upon the Metamorphosis of Tissue.*—In his work 'On the Influence of Bathing in the North Sea,' Göttingen, 1855, Beneke has communicated a series of researches which he instituted on himself, and from which he draws the following results:

In addition to its general influence in diminishing the amount of urine, sea-bathing has the specific effect, on the one side, of increasing considerably the excretion of urea and sulphuric acid, and, on the other, of diminishing at the same time the excretion of phosphoric acid. The first of these results is an indication of augmented metamorphosis of tissue; the second is an indication of the gain of the body in organic material. Moreover, residence in the sea air acts by augmenting the amount of urine, its influence on the most important constituents of the urine being the same as that of sea-bathing, except that the diminution of phosphoric acid is still more marked.

Let us now examine whether, and to what extent, this result is really founded on the numbers adduced.

With the view of determining the effects of a residence inland, Beneke had instituted at Oldenburg a series of researches, previous to those at Wangeroge, of five, fourteen, and four days respectively, viz., from the 5th to the 10th of January, from the 8th to the 21st of February, and from the 5th to the 8th of July; and after his return from Wangeroge (which was on the 17th of August) two series of researches of three days each, viz., from the 29th to the 31st of August, and from the 12th to the 14th of September. For the determination of the influence of the sea air, a series of observations of four days, from the 13th to the 16th of July, was undertaken, and for that of the sea-bathing two series, one of seven days, from the 17th to the 23d of July, and one of eight days (in which, however, no bath was taken on two of the days), from the 4th to the 11th of August.

In order to determine the influence of the sea air, Beneke compares the four days (from the 13th to the 16th of July) in Wangeroge, not with the three series of Oldenburg, but only with the last of them, with the four days in July, on the supposition, it is true, that its result does not differ to any important extent from that of the first two series. But we shall see that, instead of doing so, the latter rather tend to negative the results of the July series of observations than otherwise. That the comparison of two series of

observations of four days each cannot afford the least ground for a conclusion, has been already indicated. And the author in fact regrets that he was unable to prolong his investigations any further, but adds (page 45), that, nevertheless, the results he has obtained are reliable! Let us see how far his ideas of reliability extend.

The Means of the two series for the elements above referred to are—

	Amount of Urine.	Urea.	Sulphuric Acid.	Phosphoric Acid.
Oldenburg .	1317 c.c.	24.4 gr.	1.40 gr.	2.89 gr.
Wangeroge .	1469 ,,	27.5 ,,	1.68 ,,	2.38 ,,
Difference . .	+152 c.c.	+3.1 gr.	+.28 gr.	—.51 gr.

It was the amount of this difference which rendered it unnecessary, in his opinion, to institute further researches for the purpose of verifying the conclusion that residence at the seaside increases the gross quantity of the urine, as well as the urea and sulphuric acid, whilst it diminishes the phosphoric acid. How little, however, this difference is to be relied on, will appear if we note the inequality of the numbers from which these Means have been drawn.

We will first deal with the amount of the urine. It is not, indeed, upon the influence of the sea on the general urinary secretion that the principal emphasis is laid, the increase or diminution in the latter being for the most part attributed to diminished or increased cutaneous transpiration; yet it is interesting to observe the way in which the author deals with his numbers.

The four observations from which the Means 1317 is drawn are—

937. 1789. 1432. 1112;

those from which the Mean 1469 is deduced are—

1339. 1452. 1526. 1560.

The most cursory glance will show how little inference could be drawn from these very unequal numbers as to what the following ones would have been if the observations had been continued some days longer, and, therefore, what small ground there is for assuming

that any succeeding numbers might not have entirely inverted these relations. Indeed, if we want a striking proof of the unreliability of the author's conclusion, all that we have to do is to take the following seven days' series of investigations on the action of sea-bathing. The urinary quantities in this series are as follows:

1624. 1561. 1402. 1362. 959. 1198. 927.

If Beneke in this case had contented himself with four days' observations, the mean would have been 1487. By an application of the same ready reasoning he would have concluded a considerable increase to have occurred. But the three following days depress the Mean to 1290, and the conclusion at which he actually arrives at the end of the seventh day is that bathing has diminished the secretion.

This diminution he attributes to increased cutaneous transpiration; but if this explanation is to pass muster, we must assume that in the first four days, which offer so contradictory a result, the cutaneous transpiration was not only unusually small, but considerably less than in the last three days, for which the concomitant circumstances of the case offer no justification at all. The author shows the relation of the amount of urine to the sum of the fluids daily ingested, and for that purpose adds together without any distinction all the liquids drunk, such as water, coffee, milk, wine, &c. It might possibly be thought that the very low numbers in the last half of the above seven days' investigation depended upon an exceptionally small quantity of drink taken; but this is not the case, for on the days on which the urine was smallest we find very considerable fluid ingesta; and we therefore see that series of four, or even seven days of the author's manner of living admit of *no conclusion at all* being drawn from them with regard to the general amount of the secretion.

It would be quite superfluous to examine the remaining investigations for further confirmation of this opinion on the author's conclusions. It is only necessary to add that the Means of the series of five days in January, the fourteen days in February, and the eight days in August, are respectively 1226, 1433, and 1440, and that, so far as the last two numbers are concerned, the result of February's observations in Oldenburg entirely corresponds with that of the second of them.

The observations themselves, therefore, negative Beneke's con-

clusion, if in comparing them we refer to the earlier ones. If, however, it is considered impossible to compare them with one another in consequence of their having been undertaken at so widely distant periods, and at different seasons of the year, it seems quite superfluous to have undertaken them at all.

Before proceeding to the consideration of the individual elements of the urine, let us note, with the view of throwing some preliminary light upon the inequalities that occur, that Beneke observed no kind of uniform dietary during his observations. He neither took, on the several days, food in equal quantities or of a uniform character, but as chance directed, or his appetite suggested, at the same time assuming that any compulsion or restraint on his appetite would have caused greater disturbances than the irregularities which were occasioned by the demands of nature. But even allowing that he was to a certain extent justified in this respect, his manner of living whilst at Wangeroge was too different from that at Oldenburg to have been without influence. Whilst there, in consequence of a more vigorous appetite, he made a fuller dinner, took a supplementary breakfast, and, above all, drank more wine. The consequence of this must have been infallibly a more active metamorphosis of tissue, which, in fact, the author does not deny. But he attributes his increased appetite to residence in the sea air and sea-bathing, so as to be able to ascribe the variations in the metamorphosis of tissue to the same conditions. Why may not the more frequent and unrestricted exercise in the open air, as well as the stimulus of greater social intercourse, have been the causes? That these elements did not produce the effects in question remains to be proved. The pith of Beneke's investigations, however, consists in "the peculiar relations" which he asserts that he found in the elements of the urine, and which he considers to be the "specific" effect of the sea air and bathing. Only, in the first place, the proof that exposure to the open air inland, under similar conditions, would not have produced the same peculiarities is deficient; and, in the second, his observations, as we shall directly see, in no way bear out the presence of the peculiar conditions to which he refers.

We will commence with testing the results obtained on the subject of urea. What first imports in reference to the series of observations which were intended to determine the effects of the sea air is, that they allow us to draw no conclusion whatever from them, since they are only four in number; and I will give here the numbers

obtained, just to show that their inequality dissipates every idea of comparison between them.

The numbers are—

Wangeroge:	24.8.	30.	28.5.	26.8.	Mean, 27.5.
Oldenburg:	23.7.	28.6.	24.4.	21.	Mean, 24.4.

It will be seen that two numbers in the upper series (viz. 24.8 and 28.5) so nearly agree with two numbers of the lower one (viz. 24.4 and 28.6), that the whole of the author's conclusion (viz. that the sea air produces an increase) only rests upon two observations.

Let us now examine the numbers of the bathing period. The seven days in July gave: 27.93, 27.79, 28.39, 28.17, 29.61, 28.97, 27.51. The successive Means of which are 27.86, 28.04, 28.07, 28.38, 28.48, 28.34. Moreover, the eight days in August gave: 26.83, 25.99, 28.33, 28.65, 28.99, 32.13, 28.29, 27.81. Whose successive Means are: 26.41, 27.02, 27.45, 27.76, 28.49, 28.46, 28.38.

In consequence of the too great incompleteness of the July series of researches at Oldenburg, no other numbers remain but the present ones with which to compare the undoubtedly very far separated February series at Oldenburg, which consists of the following numbers: 22.33, 24.09, 25.26, 28.51, 27.99, 25.76, 26.80, 29.27, 24.90, 25.04, 23.35, 20.33, 22.70, 23.03; and gives the following Means: 23.21, 23.89, 25.05, 25.64, 25.66, 25.82, 26.25, 26.10, 26.00, 25.75, 25.30, 25.10, 24.95.

The predominance of high numbers in the two Wangeroge series appears at first sight to bespeak a favorable result for the increase of urea; yet this is made very doubtful by the great fluctuations in the comparative series at Oldenburg. And since these fluctuations are very evident even in the Successive Means, these latter are quite insufficient for any definite conclusion, and we are compelled to fall back on the mean fluctuations. Now the mean fluctuation of the first of the three series is .68; of the second, 1.69; of the third, 2.46. The difference of the Arithmetic Means of the *first* and *third* series is, consequently, 28.34 — 24.95 = 3.39, and the sum of the mean fluctuations .68 + 2.46 = 3.14; for the *second* and *third* series, on the other hand, the mean difference is 28.38 — 24.95 = 3.43, and the sum of the mean fluctuations 1.69 + 2.46 = 4.15. If, in consequence

of the incompleteness of the single sea-bathing series, we combine the two, we get, as an indication of their value, 28.4 as the Mean, and 1.29 as the mean fluctuation; so that a comparison with the third series gives 3.39 as the mean difference, and 3.75 as the sum of the mean fluctuations.

It is obvious that the first bathing series alone, when considered independently of the others, allows us to infer an increase; whilst the second series, and the combination of the two series, which is so much more trustworthy, renders this increase doubtful. It must be remembered, too, that the series in February was instituted in a completely different season of the year, which must make any comparison with it excessively uncertain; also, that if we were only to employ the first eleven or twelve of the fourteen days in February, a preponderance of the fluctuations over the mean difference would again result; and, finally, that if we use the useless series of four days in July, of which Beneke has availed himself, a preponderance of the fluctuations also occurs. But, even if the observations were more certain in their nature than they are, the increase—which, in the altered manner of living, is certainly very likely—might be in no way chargeable to the sea-bathing.

The case of the other elements of the urine is more unfavorable still. The observations on sulphuric acid, for instance, in the two series of four days, whose means were given above as 1.40 and 1.68, are respectively—

1.25. 1.82. 1.44. 1.10;

and

1.45. 1.93. 1.66. 1.68;

and ought the more entirely to be discarded, since here, too, two numbers of the upper series, viz. 1.82 and 1.44, so nearly coincide with two of the lower ones, viz. 1.45 and 1.93. Moreover, the numbers of the seven and eight days' bathing series are, respectively—

1.83. 1.80. 1.72. 1.94. 2.02. 1.90. 2.04;

and

1.56. 1.62. 1.74. 2.02. 1.93. 2.26. 1.90. 1.84;

and their Successive Means are—

1.81. 1.78. 1.81. 1.86. 1.87. 1.89;

and

1.59. 1.64. 1.73. 1.77. 1.85. 1.86. 1.86.

Beneke, who assumes the last Means, 1.89 and 1.86, as the standards, and compares them with the above Mean, 1.40, of the four days in Oldenburg, considers the increase of the sulphuric acid as decided, and as much more marked than he supposes to be the case under the action of the sea air alone. Let us, therefore, compare these members with those of the fourteen days' series in February, which are—

1.50.	1.62.	1.94.	2.01.	1.88.	1.84.	1.80.
1.77.	1.58.	1.58.	1.65.	1.46.	1.55.	1.51;

of which the successive Means are—

1.56.	1.69.	1.77.	1.79.	1.80.	1.80.	1.80.
1.76.	1.74.	1.73.	1.71.	1.70.	1.69.	

Hence it will be seen that the last Mean, 1.69, coincides with that of the four days in the sea air, and that no difference can be perceived between the residence at the sea and at Oldenburg, if we consider the ordinary Means as the standard, and assume the four days' series in July at Wangeroge as reliable. Moreover, it is clear that the Mean of the first five, six, and seven days of the series in February is uniformly 1.80, and consequently that the Mean which the sea-bathing periods give almost coincides with the Mean of an equal number of days of the Oldenburg series. It is quite unnecessary that our examination should be carried any further, to lead us to the conviction (quite irrespective of the fact that Beneke's process for determining sulphuric acid leaves room for error to the extent of 10-20 per cent.) of the complete groundlessness of the assertion that sea-bathing affects the excretion of sulphuric acid.

It now only remains for us to consider the results which Beneke obtained with regard to phosphoric acid, and which he deems to be of especial value. He concludes from the above two Mean numbers, which he has extracted from the two series of four days' researches in July, that the sea air actively restrains the elimination of phosphoric acid. These series are as follows:

2.78. 3.47. 2.82. 2.50;

and

2.13. 2.34. 2.39. 2.65.

Although we have shown in the previous sections that four observations alone can justify no conclusion, we have not omitted to introduce these numbers here, in order that we may call attention to

the fact that the above series, which gives a high Mean, includes the number 3.47, which must have been due to some special disturbing influence, for in no other case amongst all Beneke's other determinations of phosphoric acid do we again meet with so high a number. The highest which occurs is 2.90, and this is not in the Oldenburg observations, but in those taken during sea-bathing—in the very circumstances, that is to say, where the author asserts that he has noticed a diminution of the phosphoric acid. Moreover, the numbers of the second series, which give the smallest Mean, exhibit an unbroken increase, and this increase is continued in the sea-bathing series which is directly connected with it; so that a still stronger doubt must arise as to the correctness of Beneke's assertion, unless we feel inclined to overlook the excessive incompleteness of the series. But the case of his assertion with regard to the effects of sea-bathing is as bad as it can be; and it is quite necessary in considering it to specially examine the individual investigations, since the mean numbers themselves offer the most striking proof of its incorrectness. The Means of the seven and eight days' researches on the influence of sea-bathing are 2.62 and 2.67, whilst the Means of the January and February series of observations at Oldenburg are respectively 2.34 and 2.40; and yet, according to Beneke, the most marked effect of sea-bathing is in diminishing the elimination of phosphoric acid!

Indeed, the observation of Dr. Boecker, which he made when he handed Beneke's paper to me, may be fully confirmed, that not one of the results therein contained appeared to him to have any foundation—at least, so far as the part relating to the investigations on the urine was concerned.

Beneeke found, after his residence in Wangeroge, that his bodily weight was considerably increased. It is manifest, therefore—and that without any necessity for chemical proof—that, whilst there, he ingested more nitrogenous material as well as phosphoric acid than he excreted. Moreover, it can hardly be doubted that the richer dietary at Wangeroge not only contained more nitrogen, but also more phosphoric acid than that at Oldenburg; and the actual result does not in any way contradict the assumption, that not only was more of both substances assimilated, but also more eliminated, since the excess of assimilation may easily have exceeded the excess of elimination. That a special diminution of excretion should have been maintained, not for both, but only for one of these substances,

is too exceptional a supposition to be adopted, without corroboration by a specially instituted and scrupulous series of researches. The numbers here brought together do undoubtedly lead to the supposition, although they do not prove, that both of these substances were excreted in increased quantities.

(*b*) *Examination of some investigations by Boecker.* We will first consider a series of observations made during four days by Boecker, on a single individual, with the view of determining the action of sarsaparilla, and afterwards those which refer to the amount of urine excreted. A superficial glance at the numbers and their ultimate Means enables us at once to perceive that they do not exhibit any obvious influence upon them by this medicinal agent; but our object is not so much to corroborate or impugn the results he has drawn from them, as to use them as an illustration of the complete treatment of the figures.

The two series of observations in question have been quoted in Section I, and will be again produced for the sake of ready comparison.

The observations, during the employment of sarsaparilla, were—

(*a*)	1467.	1744.	1665.	1225.	1161.	1369.
	1675.	2129.	887.	1643.	934.	2093.

The series of normal observations was—

(*b*)	1263.	1740.	1538.	1526.	1387.	1422.
	1754.	1320.	1809.	2139.	1574.	1114.

The successive Means of the first series are—

1606.	1625.	1524.	1451.	1438.	1472.
1554.	1480.	1496.	1445.	1499.	

Those of the second are—

1501.	1514.	1517.	1491.	1479.	1519.
1494.	1529.	1590.	1588.	1549.	

The fluctuations in series (*a*) and (*b*) will be seen to be very considerable, and even those in the Successive Means are sufficiently great to enable us to conclude that the obviously evident periods in these series are not the only ones to be determined, but that longer periods, or considerable and irregular disturbances, must have operated. This would, therefore, be a case in which, in order to

determine the degree of uncertainty of the Mean, it would be necessary to estimate exactly the entire mean fluctuation, unless the Successive Means themselves indicated that the sarsaparilla had *not* exercised any sensible influence on the amount of urine; for the difference of the ultimate Means, $1549 - 1499 = 50$, is much less than the fluctuations of the successive Means amongst themselves. We will now determine the mean fluctuations for the sake of the opportunity of exhibiting to those readers who are unskilled in numerical proceedings an example of the way in which this is done. For this purpose we first determine the individual fluctuations. In the first series, whose Mean is 1499, these are—

$$
(c)\qquad
\begin{aligned}
1499 - 1467 &= +\ 32\\
1499 - 1744 &= -\ 245\\
1499 - 1665 &= -\ 166\\
1499 - 1220 &= +\ 279\\
1499 - 1161 &= +\ 338\\
1499 - 1369 &= +\ 130\\
1499 - 1675 &= -\ 176\\
1499 - 2129 &= -\ 630\\
1499 - 887 &= +\ 612\\
1499 - 1643 &= -\ 144\\
1499 - 934 &= +\ 565\\
1499 - 2093 &= -\ 594
\end{aligned}
$$

In the second series, whose Mean is 1549, the fluctuations are—

$$
(d)\qquad
\begin{aligned}
1549 - 1263 &= +\ 286\\
1549 - 1740 &= -\ 191\\
1549 - 1538 &= +\ 11\\
1549 - 1526 &= +\ 23\\
1549 - 1387 &= +\ 162\\
1549 - 1422 &= +\ 127\\
1549 - 1754 &= -\ 205\\
1549 - 1320 &= +\ 229\\
1549 - 1809 &= -\ 260\\
1549 - 2139 &= -\ 590\\
1549 - 1574 &= -\ 25\\
1549 - 1114 &= +\ 435
\end{aligned}
$$

The squares of the fluctuations are—

SERIES (c).		SERIES (d).	
$32^2 =$	1024	$286^2 =$	81796
$245^2 =$	60025	$191^2 =$	36481
$166^2 =$	27556	$11^2 =$	121
$279^2 =$	77841	$23^2 =$	529
$338^2 =$	114244	$162^2 =$	26244
$130^2 =$	16900	$127^2 =$	16129
$176^2 =$	30976	$205^2 =$	42025
$630^2 =$	396900	$229^2 =$	52441
$612^2 =$	374544	$260^2 =$	67600
$144^2 =$	20736	$590^2 =$	348100
$565^2 =$	319225	$25^2 =$	625
$594^2 =$	352836	$435^2 =$	189225
Total	1792807	Total	861316

The Means of these squares are—

$$\frac{1792807}{12} = 149401 \text{ and } \frac{861316}{12} = 71776,$$

and finally,

$$\sqrt{149401} = 386, \text{ and } \sqrt{71776} = 268.$$

Therefore, the mean fluctuations indicated in Section VII, by μ and ν, are 386 and 268; and their sum, 674, is, consequently, considerably greater than the mean difference, $1549 - 1499 = 50$, which is more than sufficient to indicate a negative result.

Of the elements of the urine, it is only necessary for us to refer to the potash and phosphoric acid; to the potash, because it exhibits the greatest mean difference, and therefore offers the greatest probability of a positive result; and to the phosphoric acid, on account of the interest which this element possesses.

For the potash the series of normal observations is—

(e)	1.55.	2.22.	1.67.	2.52.	1.38.	2.75.
	3.81.	1.56.	.30.	.51.	.89.	1.00;

and that of the sarsaparilla observations—

(f)	3.55.	2.81.	3.38.	1.79.	2.02.	2.58.
	3.56.	3.10.	2.25.	1.85.	3.29.	4.56.

The successive Means of series e are—

(g)	1.88.	1.81.	1.99.	1.87.	2.01.	2.18.
	2.10.	1.90.	1.76.	1.68.	1.63;	

and of series f—

(h)	3.18.	3.25.	2.88.	2.71.	2.68.	2.81.
	2.85.	2.78.	2.70.	2.74.	2.89.	

Whilst the Means in series (h) are moderately uniform, those in series (g) exhibit in its second half greater fluctuations than in its first; and we should, therefore, assume that series (e) had been too much influenced by disturbances, and was unfitted for purposes of comparison, even if the following considerations did not otherwise compel us to do so. This series (e) is divisible into two distinct parts: the first eight days give high values, whose successive Means exhibit but little fluctuation; whilst the last four days descend suddenly to very low ones. Moreover, a comparison of this series of observations with others teaches us that it is on the low numbers that the disturbing agency has operated; the probability of which assumption is increased when we consider that the days during which the observations of series (f) were carried on immediately followed those of series (e), and, consequently, that the quantity of potash must have risen quite suddenly from 1.00 to 3.18, 3.25, &c. In addition, the smaller numbers regularly increase from the first member of the group, .30.

Let us, therefore, assume that the first eight days are alone of any comparative value; to which there will be the less objection as, in doing so, we reject the lowest, and those numbers which are most favorable for a positive result.

We will now, as a preliminary, estimate the amount of uncertainty from the Successive Means, in order to see whether it will be worth our while to take its more accurate gauge by means of the mean fluctuation. The Mean of the first eight numbers of series (g) is 1.97; the greatest divergence from the Successive Mean, .21; the Mean of series (h) is 2.86, and the greatest divergence .39; consequently, the difference of the mean values ($2.86 - 1.97 = .89$), is greater than the sum (.6) of the greatest divergences, which gives us a prospect of a positive result. In reality, the mean fluctuation in the first eight numbers of series (e) is .42, and that of series (f) .64, so that the mean difference ($2.89 - 1.90 = .99$)

so nearly coincides with the sum of the mean fluctuations (1.06), that an increase of potash, as a result of the use of the sarsaparilla, appears probable, which certainly may be caused by an unusually large amount of potash in the drug itself.

Finally, the normal series for the phosphoric acid is—

(*i*)	1.46.	1.36.	1.36.	1.51.	.94.	1.36.
	.96.	1.03.	.76.	1.04.	1.76.	1.17;

and that for the sarsaparilla—

(*k*)	1.23.	1.33.	1.29.	.70.	1.57.	1.51.
	1.62.	1.79.	1.37.	1.49.	1.43.	1.93.

The Successive Means of series (*i*) are—

(*l*)	1.41.	1.39.	1.42.	1.33.	1.33.	1.26.
	1.25.	1.19.	1.18.	1.23.	1.23;	

whilst those of series (*k*) are—

(*m*)	1.28.	1.28.	1.14.	1.22.	1.27.	1.32.
	1.38.	1.38.	1.39.	1.37.	1.41.	

It will be seen that the numbers in the two series repeatedly fluctuate backwards and forwards, whilst the Means of the first series exhibit an apparently unbroken decrease, and those of the second series an apparently unbroken increase: hence we must assume, first, that disturbing influences, of a short periodic duration, have come into play, whose action has to a great extent been reciprocally equalized amongst the Means; and, secondly, that the first series of Means points to the *decreasing* portion of a long periodic disturbance, whilst the second series of Means points to the *increasing* one. Moreover, thirdly, we are led to suspect that the two series of observations followed immediately the one on the other, and that the limits of their Means were closely related—that both of them belonged to one and the same period; and, fourthly, to conclude therefrom, that both halves fluctuated within nearly the same limits, and that the sarsaparilla exercised no share, or at least an inconsiderable one, in the influence on the Means.

Let us now examine some other of Boecker's researches, especially those which refer to the influence of sugar and alcohol upon the amount of urea in the urine ('Contributions to Medical Science.' Crefeld, 1849).

The researches on sugar extended over nine days, and gave for the amount of urea in the urine the following figures :

14.1. 27.8. 19.2. 13.8. 14.9. 17.8. 14.5. 18.5. 16.2.

The normal series of observations was conducted for four consecutive days, shortly before the employment of the sugar, and for ten consecutive days after it, and gave the following numbers:

30.6. 31.2. 24.2. 24.3. 33.8. 22.6. 27.9.
17.7. 23.2. 22.9. 17.8. 18.5. 23.0. 25.0.

The second number in the first series differs so widely from all the others that it is most convenient to exclude it. The remaining eight observations give 15.7 as their Mean, with the mean fluctuation of 2.03; whilst the Mean of the normal series is 24.5, and the mean fluctuation 4.76. The difference of the Means is, consequently, 8.8, and the sum of the mean fluctuations 6.79; from which a decided diminution of urea, under the influence of sugar, cannot be predicated.

Alcohol was taken during six days, and the numbers obtained were—

18.48. 20.02. 14.57. 22.54. 17.04. 22.37;

whose Mean is 19.17, and the mean fluctuation 2.84. The corresponding normal series of observations, whose number was nine, and a portion of which was instituted during very distant periods of time (they extended in separate groups from the 17th of November to the 6th of February; whilst the alcohol series occurred between the 17th and the 26th of February), gave the following numbers:

27.85. 26.92. 28.34. 35.24. 38.75.
31.17. 24.39. 25.04. 35.36;

whose Arithmetic Mean is 30.34. Since the smallest number in the normal series exceeds the largest in the alcohol one, the diminution by the use of this agent is obviously demonstrated.

Let us examine in the same manner the amount of earthy phosphates contained in the urine. During the use of sugar, the numbers obtained were, if we exclude, as above, the second number—

.94. 1.00. .85. .89. 1.08. 1.08. .62. .75.

In the normal series the earthy phosphates were determined during the first five days, and the numbers obtained were:

2.64. 2.60. .96. 1.58. 2.54.

The first series gives the Mean .93, with the mean fluctuation .15; the second series gives the Mean 2.06, with the mean fluctuation .68. The mean difference is, therefore, 1.13, and the sum of the Mean fluctuations .83; consequently, as the first number consider-

ably exceeds the second, we may assume that the sugar has probably produced a diminution of the earthy phosphates, if we allow the five normal observations to be available for comparison. It deserves, however, to be remarked that the assumption of a diminution requires confirmation, if we compare the results of the sugar series with those of the alcohol one, instituted nine to twelve months earlier. These latter give the numbers—

2.39. 1.18. 2.01. 2.82. 2.71. 2.96. .94. 1.35. 1.59;

which give the Mean 1.99, with the mean fluctuation .72. Hence the mean difference will be 1.06, and the sum of the mean fluctuations .87; and, consequently, the first number is again greater than the second.

The alcohol series gave as the numbers of the earthy phosphates

2.47. 1.28. 1.11. 1.33. 2.04. .99;

of which the Mean is 1.54, and the mean fluctuation ·53. For the corresponding normal series we get as the Mean 1.99, and as the mean fluctuation .72. The mean difference is, consequently, .45, and the sum of the mean fluctuations 1.25; so that the inference drawn from the Means alone, viz., that the earthy phosphates have diminished, receives no confirmation whatever from a consideration of the fluctuations.

Let us, finally, take the entire amount of the solid matters of the urine. The quantities obtained in the alcohol series were—

50.69. 45.70. 45.60. 69.64. 58.11. 54.26.

In the corresponding normal series they were—

73.07. 66.70. 73.80. 98.90. 94.72.
101.66. 69.76. 65.12. 100.02.

In the sugar series—

57.68. 53.64. 59.82. 59.24. 55.40.
57.82. 52.39. 54.04. 51.03.

And in the corresponding fourteen days' normal series—

67.37. 81.72. 59.74. 63.54. 78.97. 66.00. 79.94.
59.16. 78.19. 82.43. 63.23. 62.92. 73.75. 60.18.

In the last series, indeed, the fluctuations are somewhat great, but the very uniform successive Means (viz., the series—

74.54. 69.61. 68.09. 70.27. 69.56. 71.04. 69.56.
70.51. 71.71. 70.94. 70.27. 70.77. 69.79)

prove that the readily recognisable and but slightly varying period of two days, in the observations which immediately followed one another, absorbs the greatest portion of the fluctuations; and we are not only safe in using the series, but we shall, in our estimate of the mean fluctuation, obtain a sensibly greater measure of uncertainty than really exists. Nevertheless, this excessive estimate allows us to recognise a distinct diminution of the solid constituents of the urine under the use of sugar, just as an unequivocal diminution of them is evident in the alcohol series. The Arithmetic Means of the mean fluctuations are, respectively—

Alcohol	54.00	and	8.54.
„ (normal series) .	82.64	„	14.80.
Sugar	55.67	„	2.94.
„ (normal series) .	69.79	„	6.40.

The mean difference and sum of the fluctuations is, therefore—

Alcohol	28.64	and	23.34.
Sugar	14.12	„	9.34.

III. *The researches of Kaupp on the action of Chloride of Sodium.*—These researches (published in the 'Archiv für Physiologische Heilkunde,' 1855) are of very great value, both on account of the great continuity of the periods which they embrace, and of the uniformity of the manner of living during the time they were carried on, in reference to which point extreme care in the chemical analyses was taken. For this purpose, during the eighty-seven days of observation the solid food as well as the fluids taken were every day quantitatively and qualitatively arranged, with the exception of twelve days, in which the effect of the most complete abstinence possible from salt was observed, and in which, on that account, a slight change was necessary. Thus, for instance, the ham sausages which were daily consumed, on account of the chloride of sodium they contained, were mixed with an equal weight of fresh pork; and as the mid-day soup was not very palatable when prepared without salt, its (weighed) constituents were cooked in another form, and the water which would otherwise have been used in their preparation drunk with them. The hours of each meal, of rising and going to bed, and of satisfying the wants of nature, were rigorously maintained on every day alike; labour was uniformly distributed throughout the day; walks, mechanical employments, &c., were avoided; the temperature

of his room was maintained in constant uniformity with that of the external air, and the mean temperature noted at intervals. Equal care was bestowed upon the chemical determinations. After such exactitude in the process of investigation, we may readily assume that the results which the author deduced from it are admissible; and it is for this reason that I here undertake to examine some of them by way of example.

The investigations lasted, almost without interruption, from the 28th of April to the 28th of July, and are divisible into six series of twelve days each, and one of fifteen days. During the series there was taken daily of common salt, successively, 0 gramme, 5 gr., 10 gr., 15 gr., 20 gr., 25 gr., 30 gr.; or, including that contained in the food, 1.5 gr., 9.3 gr., 14.2 gr., 19.0 gr., 23.9 gr., 28.7 gr., 33.6 gr.

I will first dispose of the series of observations on the quantities of the urine (corresponding to those of salt ingested), with their successive Means, and the Probable Means deduced from these latter. Since the differences between the Successive Means seldom reach or exceed 8 per cent., the Arithmetic Mean of the last eight observations is in round numbers assumed as the *Probable Mean*, and the greatest fluctuation between them as the *uncertainty*; by which assumption the uncertainty will certainly be more likely to be rated too low than too high.

(1) Chloride of sodium, grammes[1] 33.6.

Observations:

2088.	2330.	2040.	2526.	1900.	2375.
2225.	2356.	2680.	2500.	2530.	2166.

Successive Means.

2209.	2153.	2246.	2177.	2210.	2212.
2230.	2280.	2302.	2323.	2310.	

Probable Mean 2250 ± 70.

(2) Chloride of sodium, grammes 28.7.

Observations:

2025.	2590.	2280.	1802.	2864.	2505.
2323.	2205.	2904.	1820.	2119.	1900.

[1] 1 gramme = 15.432 grains (English).

Successive Means.

2308.	2298.	2174.	2312.	2361.	2341.
2324.	2378.	2323.	2304.	2271.	

Probable Mean 2330 ± 60.

(3) Chloride of sodium, grammes 19.

Observations:

2830.	2615.	2010.	2270.	1905.	2092.	1700.	2415.
2222.	2404.	2387.	2225.	2340.	2248.	1602.	

Successive Means.

2722.	2485.	2431.	2326.	2287.	2203.	2230.
2230.	2246.	2259.	2256.	2263.	2262.	2218.

Probable Mean 2250 ± 50.

(4) Chloride of sodium, grammes 14.2.

Observations:

1590.	1765.	1904.	1774.	1600.	1862.
2540.	2200.	2576.	2435.	2180.	2256.

Successive Means.

1677.	1753.	1758.	1726.	1749.	1862.
1904.	1979.	2025.	2039.	2057.	

Probable Mean 1920 ± 190.

(5) Chloride of sodium, grammes 9.3.

Observations:

2800.	2980.	2905.	2704.	2312.	2680.
2206.	2580.	2445.	2176.	2114.	2506.

Successive Means.

2890.	2895.	2847.	2740.	2730.	2655.
2646.	2624.	2579.	2537.	2534.	

Probable Mean 2630 ± 110.

(6) Chloride of sodium, grammes 1.5.

Observations:

2488.	2080.	2121.	2100.	2048.	1900.
2225.	2665.	2004.	2200.	1956.	2206,

Successive Means.

2284.	2230.	2197.	2167.	2123.	2137.
2203.	2181.	2183.	2162.	2166.	

Probable Mean 2170 ± 50.

(7) Chloride of sodium, grammes 23.9.

Observations.

2200.	2696.	2650.	2272.	2720.	2335.
2280.	2090.	2422.	1992.	2350.	2611.

Successive Means.

2448.	2515.	2454.	2508.	2479.	2450.
2405.	2407.	2366.	2364.	2385.	

Probable Mean 2420 ± 80.

If we compare these Probable Means with one another, we shall see that, notwithstanding that the amount of uncertainty is less than a rigorously accurate investigation demands, except in the fifth series, the difference between two mean numbers is in no case greater than the sum of their uncertainties; and that, therefore, no conclusion can be drawn from any of these series as to an increase or a diminution of the urine. The Probable Mean of the fifth series exceeds in respect of its uncertainty those of all the others; but this cannot be ascribed to the salt, for the quantity in this case (9.3 gr.) was not extreme, and the high numbers obtained are found only in one (the first) half of the series; and still less to the temperature, since some of the other series were instituted under a higher, and others under a lower, temperature. On the other hand, the first three days are noted as having been especially damp, and those days in the other series which figure as damp occur for the most part in combination with increased excretion; so that it is not altogether unreasonable to consider the dampness as the cause of the unusual disturbance in that series, especially as its tendency would be to diminish the other great means of elimination—transpiration by the skin.

Let us compare with these conclusions those which Kaupp himself has drawn from his investigations. He states, in the first place, that "a reference to the Means shows that, as a rule, large doses of salt gave an increased amount of urine, with some striking exceptions. The mean amount of urine for the three series of observations

in which the largest amount of salt was ingested was 2324 cub. cent.[1] per diem, at a temperature of 10.8° (56.3° Fah.); and for the four series in which it was smaller only 2238 cub. cent., but at a temperature of 15.6° (67.1° Fah.). The difference of temperature is here obviously so great that, putting aside other reasons, any conclusion would be unjustifiable." Kaupp would, therefore, have considered the mean numbers 2324 and 2238 as sufficiently remote from one another to admit the conclusion that an increase of the urine resulted from an increase of the dose of salt (although this difference, as appears above, is less than the small estimated measure of uncertainty of several of the other series), if the consideration of the temperature had not prevented him from doing so. Indeed, though he does not admit these numbers, he avails himself of them afterwards in order to correct them on the point of temperature. With the view of eliminating the influence of temperature, he collocates in each series the observations of those days whose mean temperatures approximate to one another, and especially those in which the temperature lies between 7° and 9°, between 9° and 11°, between 11° and 13°, and so on, to between 17° and 19°. Thus, for example, we have, for the temperature of 7°—9°, in the first series, five days (viz., the first, second, ninth, tenth, and twelfth days), with a mean volume of urine of 2353; and in the second series, three days (viz., the third, seventh, and ninth days), with the mean volume of 2502. For the sake of explaining this point more clearly, I will cite the first three groups, arranged according to their temperature:

Temperature. Reaumur.	Dose of Salt.	Mean Volume of Urine.	Number of Days.
7°— 9°	33.6	2353	5
	28.7	2502	3
9°—11°	33.6	2288	6
	28.7	2249	8
	9.3	2722	4
11°—13°	28.7	2009	2
	23.9	2510	3
	14.2	2422	3
	9.3	2630	2
	&c.	&c.	&c.

[1] 16.386 cubic centimetres = 1 cubic inch (English); or 1 cubic centimetre = ·061027 of a cubic inch.

From these numbers the general conclusion will be evident that (in opposition to the one previously drawn) *large* doses of common salt cause a *diminution* of the urine, since the (not infrequent) exceptions in the table may be ascribed to the small number of observations made.

With regard to the utility of the numbers thus obtained, it must be remarked, 1st, that the number of days compared is much too small to allow us to consider their Mean as of any value; and, 2d, that, in addition, the days compared are not consecutive to one another, and, consequently, the observations are unconnected, so that it is impossible to say that there is any comparison of the brief periods of disturbance which are very probably present. Moreover, it will be seen that in the second group, in which the greater number of the observations (viz. 6 and 8) are compared, we have a fall from 2288 to 2249, instead of an increase.

The author then reduces the observations in the above groups to two numbers, when they do not already consist of two numbers, by adding those with large and those with small doses of chloride of sodium, respectively, to one another. Thus, for instance, the numbers in the second group, belonging to 33.6 and 28.7, those in the third group, to 28.7 and 23.9, and to 14.2 and 9.3, are united, with the following result:

Temperature.	Dose of Salt.	Volume of Urine.	Dose of Salt.	Volume of Urine.
7°— 9°	33.6	2353	28.7	2502
9°—11°	31.2	2258	9.3	2722
11°—13°	25.3	2259	11.7	2525
13°—15°	21.4	2204	1.5	2382
15°—17°	21.4	2204	5.4	2218
17°—19°	19.0	2299	5.4	2198
Mean . .	25.3	2288	10.3	2424

From this table it would be concluded, that a diminution in the dose of salt of 25.3 − 10.3 = 14.9 gr. produced an increase in the urine of 2424 − 2288 = 136 cub. cent., *i.e.*, a diminution of 9 cub. cent. for each grain of salt. It is unnecessary to remind the reader, that the result would have no claim to certainty even if the difference, 136, did not lay within the limits of uncertainty of the main series.

In order to estimate numerically the influence of the temperature, Kaupp proceeds as follows: he groups together observations according to their temperature only, in such a manner that, in the first group, all those observations are united in which the mean temperature varied from 5.3° to 8.6°; in the second group, those whose temperature varied from 9° to 10.7°; and so on, taking the Mean of each group. The result is contained in the following table, in reference to which it will be seen that the numbers included in the temperature column give the Mean of the limits of temperature, and those in the column of urinary qualities exhibit the amount of urine reduced to an equal dose of salt (calculated in a way which will be mentioned hereafter).

Temperature.	Quantities of Urine.			Mean Dose of Salt.	Number of Observations.
	Mean.	Min.	Max.		
5.3°— 8.6° (7.6°)	2430 (2555)	2088	2904	31.5	10
9.0°—10.7° (9.6°)	2367 (2435)	1802	2980	25.8	18
11°—12.6° (11.7°)	2406 (2406)	1900	2720	19.	10
13°—14.6° (14.1°)	2327 (2317)	1905	2830	18.	11
15°—16.6° (15.8°)	2218 (2154)	1700	2650	12.6	16
17°—18.7° (17.6°)	2223 (2159)	1765	2696	12.6	16
19.5°—21.3° (20.6°)	1725 (1648)	1590	1904	15.3	6

From these uncorrected numbers it will appear, since they decrease, if not consecutively, yet on the whole, that a diminution of urine occurs with an increase of temperature, and that for a rise of each degree of temperature there is a diminution of 54 cub. cent. of urine. These numbers are obtained by dividing the difference between the first and last numbers (2430—1725) by the difference of temperature, $20.6 - 7.6 = 13$.

Therefore, notwithstanding the irregularity of the intervening numbers, Kaupp considers the extremes 2430 and 1725 as standards; and, notwithstanding that the difference $2430 - 1725 = 705$ is less than the difference between the maximum and minimum in nearly all the rows, he speaks of the diminution as unmistakable! In consequence of the irregularity of the intermediate numbers,

Kaupp acknowledges that the average obtained, viz., 54 cub. cent., is still too incorrect; and reduces, since he considers the unequal doses of salt as the source of the irregularities, the aggregate Mean to correspondence with a Mean dose of salt of 19 grammes. He had found, as is mentioned above, that an increase of 1 gr. of salt produced a diminution in the urine of 9 cubic centimètres; but, since another combination gave him, instead, the number 11.9, he takes 10 cub. cent. as the diminution caused by 1 gr. of salt, and calculates the numbers in the table accordingly, which now, indeed, exhibit an unbroken, though by no means a regular, decrease. The difference of the extreme corrected numbers (2555 − 1648 = 907) divided by the difference of the corresponding Means of temperature (viz. 13) gives him, then, 70 cub. cent. as the more accurate diminution of urine caused by 1° of temperature.

We see here an uncertain number corrected by one still more uncertain. The uncorrected numbers are already very uncertain, for the reason that they are obtained from days that are often very widely separated from one another; of which the great differences between the adduced maxima and minima give confirmation. The influence of the chloride of sodium is, however, as has been shown above, entirely problematical; so that the corrected numbers are possibly much more inaccurate than the uncorrected ones. We can only conclude, therefore, that the result deduced is entirely a problematical one.

Another method of treating the influence of temperature, which is given in the paper, is decidedly superior. Each of the original series is divided into two halves, according as its members represent high or low temperatures, by which means the doses of salt will be equal on both sides, and then the Mean of the two groups so formed is taken. The results are as follows:

Series of Observations.	Mean Temperature.	Volume of Urine.	Mean Temperature.	Volume of Urine.
33.6	9.5°	2288	7.3°	2331
28.7	10.7°	2028	9°	2531
23.9	15.6°	2339	12.7°	2430
19.	18.5°	2181	15.5°	2120
14.2	20.4°	1749	12.3°	2364
9.3	15.8°	2338	9.5°	2730
1.5	17.5°	2274	15.8°	2240
Mean . .	15.4°	2171	11.7°	2421

The difference of the mean quantities of urine, 2421 – 2171 = 250, divided by the difference of the Means of the mean degrees of temperature (15.4 – 11.7 = 3·7), which gives 68 (erroneously 71 in the text) is the amount of change in the volume of the urine for 1° of temperature.

Here numbers that are not inaccurate, by being united with numbers that are so, are made still more inaccurate; and we should, therefore, view the result as one of approximate correctness, if the individual Means employed possessed the requisite degree of accuracy. Whether this latter is the case here is very doubtful, for the individual Means of the urinary quantities are deduced only from six observations, and these taken on days very remote from one another; so that their certainty, as might be expected, is very much less than that of the Means of the original series of twelve days; and even these consecutive Means are taken from different periods, in which the influences at work may have been of varying value.

The juxtaposed Means of the last table are certainly favorable to the assumption previously made as to the influence of temperature, in so far as they exhibit, in five out of the seven, a higher value for the lower temperatures; but, in the rows in which the difference is most manifest, it also happens that the unusually high numbers of the damp days above mentioned coincide with the lower temperatures; and it is also suspicious that the number 2288 on the left has nearly the same mean temperature with the number 2531 on the right, as has the number 2339 with 2130; consequently, observations instituted at a uniform temperature exhibit greater differences than the mean numbers taken for comparison from more extreme temperatures do.

The combination, too, rests upon the improbable supposition that the change of volume of the urine is proportional to the variation of temperature. The near coincidence of the result 68 with the number 70 first obtained is undoubtedly purely accidental.

If I have here argued against the assumption which would attribute a diminishing influence to temperature, it is not because I think it an improbable one, but only with the view of proving that the observations under consideration, and especially the method of determination employed, admit of no conclusion being drawn from them in reference to this point. In reality, so far as my own personal opinion is concerned, I believe on rational grounds that such an influence is probable, since everything which increases trans-

piration by the skin, such as dry warmth, ought to react upon the amount of excretion by other outlets. The careful observations of Kaupp, which greatly favour this hypothesis, must, however, only be considered as favouring, and not as proving it.

Amongst the other series of researches contained in Kaupp's paper, the measurements of urea exhibit an especial uniformity; and it is, therefore, well worth while to avail ourselves of this circumstance, which is so favorable to our obtaining a positive result, in order to see if the author is right in ascribing the fluctuations which occur in it to the temperature and to the ingestion of the chloride of sodium.

The original observations are given in the succeeding table, but only as far as their first decimals, on account of the uncertainty of their latter ones. The urea in grammes is indicated by H, the mean temperature of the day on which the observation was made by T, and the dose of salt by K.

K = 33.6.		K = 28.7.		K = 19.		K = 14.2.		K = 9.3.		K = 1.5.		K = 23.9.	
T.	H.	T.	H.	T.	H.	T.	H.	T.	H.	T.	H.	T.	H.
7.5	35.5	10.3	37.5	14.6	32.3	21.3	33.3	7.5	35.6	17.0	32.3	12.0	34.3
7.5	35.0	9.6	36.3	16.5	34.8	18.3	34.9	9.6	35.8	18.0	35.4	17.3	35.0
9.5	35.7	8.3	35.6	15.5	32.2	20.0	32.7	9.3	34.9	16.6	32.9	16.6	37.1
9.0	34.1	10.0	36.0	17.5	30.6	19.5	35.1	10.0	36.2	14.5	33.0	15.0	33.2
10.3	36.1	9.3	39.1	13.7	32.0	21.3	34.9	9.0	34.7	16.3	33.4	12.0	34.3
9.7	34.5	10.0	35.8	15.0	33.9	21.3	34.8	11.5	32.4	18.0	33.4	14.6	33.1
5.3	37.8	8.6	39.9	15.6	32.3	15.0	36.6	16.3	34.0	16.5	35.2	14.0	34.1
9.7	35.8	9.5	32.8	14.0	33.3	14.6	35.2	12.6	34.6	14.3	32.2	14.6	32.9
8.0	34.8	8.0	36.3	17.3	33.0	11.0	36.5	17.0	32.5	17.3	35.3	13.0	35.0
8.5	85.5	10.6	32.8	18.3	34.9	12.0	31.3	17.5	33.2	16.3	34.3	15.3	33.0
9.3	36.7	11.3	33.4	17.0	33.6	10.3	33.4	15.3	32.2	17.6	35.5	13.3	33.5
7.3	35.1	11.5	36.1	17.6	34.8	11.3	32.9	15.3	35.3	16.0	34.6	12.0	34.9
				18.0	34.4								
				18.6	34.8								
				21.0	29.5								

The Successive Means of these quantities, calculated to the second decimal, are as follows:

K = 33.6.	K = 28.7.	K = 19.	K = 14.2.	K = 9.3.	K = 1.5.	K = 23.9.
35.22	36.86	33.52	32.14	35.66	33.84	34.68
35.38	36.44	33.06	33.68	35.39	33.52	35.49
35.06	36.33	32.46	34.04	35.60	33.39	34.91
35.27	36.88	32.37	34.21	35.42	33.38	34.78
35.14	36.70	32.45	34.31	34.92	33.38	34.49
35.52	37.16	32.43	34.63	34.79	33.64	34.44
35.56	36.62	32.54	34.71	34.76	33.46	34.24
35.48	36.58	32.60	34.91	34.51	33.66	34.23
35.48	36.20	32.82	34.54	34.38	33.73	34.12
35.59	35.94	32.90	34.44	34.18	33.89	34.13
35.55	35.95	33.05	34.31	34.28	33.95	34.20
		33.16				
		33.28				
		33.02				

From these tables are extracted (*a*) the total Means of the individual series; (*b*) the Probable Means, drawn from the last eight Successive Means, and, as above, with the greatest divergence from them as the limit of certainty; and (*c*) the mean fluctuations calculated by the process of the mean error.

K.	Probable Value, estimated from the Successive Mean.	Arithmetic Mean, with the Mean Fluctuation.
I. 33.6	35.5 ± 0.4.	35.55 ± ·97.
II. 28.7	36.5 ± 0.7 { 36.6 ± 0.6. 34.1 ± 0.5.	35.95 ± 2.14 { 37.16 ± 1.59. 34.26 ± 1.59.
III. 19.0	32.8 ± 0.4.	33.02 ± 1.57.
IV. 14.2	34.5 ± 0.4.	34.31 ± 1.52.
V. 9.3	34.7 ± 0.7.	34.28 ± 1.24.
VI. 1.5	33.6 ± 0.4.	33.95 ± 1.17.
VII. 23.9	34.3 ± 0.5.	34.20 ± 1.01.

In the first place, it is evident that the Means of series I and II so considerably exceed those of all the others, that, in estimating the limits of error for the Successive Means, we are compelled to assume for them a decided excess in comparison with the remaining series. But as these two series correspond to the largest doses of salt and the lowest temperature, we may readily agree to regard the excess as the effect of the salt, or of the temperature, or of both agencies combined. Kaupp elects in favour of the first of these agencies, and, in order to justify his assumption, compares the Means of series I, II, and VII (in which the three highest doses of salt occur) with those of the four other series, and infers therefrom an increase of

.08 gramme of urea for each additional gramme of salt. He diminishes this number, however, to .057 gramme, since he thought that he recognised a definite diminution of the urea under a diminution of temperature, which he took into consideration in his calculations. A second calculation in which he compared the observations in which the temperature was nearly uniform (and in which he also excluded some disturbing numbers) gave him a still lower quantity, .028; and he, therefore, finally decided on .04 as the mean number.

In opposition, however, to the assumption that the excess was produced by the chloride of sodium, it must be noted that series III exhibits the next lowest dose of salt in combination with the minimum of urea; and any doubt which may exist on the subject rises to certainty when we observe that in series II only the first half exhibits a very high Mean, whilst the second half, on the other hand, gives us one which is comparable with the lower numbers given by the remaining series. The Mean of the first seven days is 37.16; that of the last five is 34.26. On this account, in the above table the two numbers are added to the value of series II, which respectively indicate the values of the first seven and last five days. The contrast between the two parts of this series is so great, that even the high mean fluctuations taken as the limits of uncertainty do not conceal the excess of the one part over the other.[1] Consequently, we are just as little justified in assuming that the high Means in I and II are the effect of the salt as of the temperature, since the first part of the second series exhibits nearly the same temperature as the lower number of the second part. The rapid transition from high to low numbers in the Means of a single series, and the gradual increase in passing from the first series into the second, which the Successive Means make very evident, together with the absence throughout of any evident leap from the numbers of one series to those of another, lead to the conclusion that the aggregate of eighty-seven days' observations constitute a continuous series, which is quite independent of the incident produced by the variation of the dose of chloride

[1] If we allow the sufficiency of the Arithmetic Mean as a standard, we may urge against the assumed action of the chloride of sodium the fact that series I has a sensibly lower mean than series II. Moreover, had Kaupp excluded the first two series, and then placed the two series with the largest doses of salt (III and VII) in juxtaposition with the three in which it was lowest, he would have obtained a contrary result, viz., the *diminution* of urea with the increase of the dose of salt!

of sodium. With the view of getting as accurate a survey as possible of the fluctuations, I undertook to make a diagrammatic table of the whole of the fluctuations about the Mean of the eighty-seven days, viz., 34.47. The vertical lines in Figure I, which is a copy of this one,[1] and in which 0.0 is the line of the general Mean, indicate the limits within which the dose of salt fluctuated. The first glance confirms unhesitatingly the suspicion expressed above of the presence of a period of fluctuation of short duration; and what is most surprising, it shows that this period is exactly one of two days. Only in four instances does this period vary by a single day; and the first of these coincides with an interruption of the observations for one day, so that, in fact, there is no variation at all; whilst in the three other instances the observations exhibit a stationary quantity. The fluctuations in these periods are, for the most part, of considerable amount; so that if we take the Means of the observations in pairs, we get a series of numbers with a sensibly uniform course. The dotted line in the figure exhibits the outline of this series of numbers (the aggregate Mean being again assumed as the basis line). This figure will render it completely clear why, in the above tables, the limits of uncertainty deduced from the Successive Means appear so much smaller than the mean fluctuations.

If we now calculate, which, after establishing the existence of a period of two days, is permissable, the mean fluctuations from the Means of the observations taken in pairs, instead of from the observations themselves, we arrive at the following results:

I. $35.55 \pm .64$.

II. 35.95 ± 1.06 $\begin{cases} 36.70 \pm .35 \\ 34.61 \pm .85. \end{cases}$

III. $33.02 \pm .86$

IV. $34.31 \pm .87$

V. 34.28 ± 1.02.

VI. $33.95 \pm .74$.

VII. $34.20 \pm .57$.

In the double numbers of series II, the upper one refers to the first six, and the lower to the last four days.

After looking at these numbers no one will be likely to ascribe any influence over the excretions of urea to the doses of salt.

If we examine the course of the observations after the elimination of the fluctuations of the period of two days, we observe that it be-

[1] This table is unaccountably omitted in the original.—*Translator.*

gins with a large wave, whose highest point is reached in the middle of series II, and whose lowest at the commencement of series IV; after which succeed smaller waves, which on an average have a duration of from six to eight days, but are independent of the variations of temperature.

With regard to the period of two days, I have always found it more or less distinctly in the case of urea, in all the series of investigations that are at my command, in which the days upon which the observations are made follow one another without any intermission.[1]

SECTION XI.—*On the employment of Quadratics for the determination of Mean Fluctuations.*

The construction of squares and square roots in the process for determining the Mean fluctuation, is even for adepts a somewhat tedious and troublesome operation. The task, however, may be considerably abridged by making use of a table of squares. That which is contained in the small table of logarithms, by August,[2] is quite sufficient for the purpose. The method of using it is given in the explanations which are appended to the tables: nevertheless, instead of the rules given there for obtaining the squares and square roots of numbers which are not contained in the tables, I will here give some which are of a much simpler character.

In the tables we have only the squares of numbers from .0 up to 2.099, the first three figures of each number standing in the first column of each page, and the fourth figure being placed over the ten following columns. The square of the number is found in the same horizontal line with that of its first three figures, and in that one of the ten columns over which the fourth figure is placed. It must be noted that the first two figures of the square, which are common to several spaces, will be found in the first column over which 0 is placed, and only the last three are to be sought for in the column over which the fourth figure of the given number is placed. For those last three figures, which are marked thus *, we must use, not the two first figures of the 0 column, which belong to the preceding numbers,

[1] In consequence of want of space, the analysis of Molcschott's paper, "On the influence of warmth on the excretion of carbonic acid in frogs," is unavoidably omitted.—*Translator.*

[2] 'Complete Trigonometric and Logarithmic Tables,' by August, 4th ed. Berlin, 1857.

but those which belong to the succeeding ones. Thus, for example, the square of .637 is not .3058, but .4058.

If we want to find the square of a number which is greater than 2.099, we shift the decimal stop as far as is necessary to make the number less than 2.099, take the square number contained in the contiguous column, and then shift its decimal stop twice the number of places that we did in the original number. For instance, suppose that we wish to find the square of 18.52; we shift the stop one place so as to convert it into 1.852, look for the square of that number, which the table gives as 3.4299, and then shift the stop in it twice as many places as we did in the first number, i. e. two places, which converts it into 342.99, or the square of 18.52. Or, if we want to find the square of 136, we write it 1.36 or 1.360. The square given in the table for this number is 1.8496. By moving the stop double the number of places that we did at first, we convert this into 18496, which is, consequently, the square of 136.

With regard to square roots, the tables only give the roots of numbers from 0 to 4.4058. If, now, we wish to find the root of any number which is less than 4.4058, e.g., 1.7505, we look for the first two figures 1.7 in the 0 space, and the last three figures in the other columns of the same or following spaces. If we cannot exactly find the last three figures (505), we take those three numbers of the table which come nearest to them (in this case 503). We, therefore, take for the first three figures of the required root the figures of the first series which stand on its horizontal line (1.32), and as the fourth figure the one which is inscribed over the column (in this case 3), so that 1.323 are the first four numbers of the root of 1.7505.

If, on the other hand, the number whose square root we wish to find is greater than 4.4058, we shift the decimal stop two, or four, or six (always an equal number of) places, until we get a number which is less than 4.4058; we then look in the table for its root, and shift the stop of the number found half as many places to the right as in the original number. Thus, if we wish to find $\sqrt{238.5}$, we write it $\sqrt{2.385}$, or $\sqrt{2.3850}$; we shall then find as the corresponding (or the nearest to the corresponding number, in this case 2.3839) number in the table the number 1.544, in which we shift the stop half as many places to the right as we moved it to the left before, which gives us 15.44 as $\sqrt{238.5}$.

Or, suppose we want to find $\sqrt{38542}$, we write it $\sqrt{3.8542}$,

The nearest number to this in the table is 38534, of which the root given in the table is 1.963. We shift the stop two places, and get 196.3 as $\sqrt{38542}$.

By the help of these tables the mean fluctuation may be found with very little trouble.

SECTION XII.—*Conclusion.*

In the preceding pages I believe that I have exposed, in a way that will be intelligible to medical readers who are not proficient in mathematics, the principles upon which conclusions should alone be deduced from any given series of observations, and that I have placed them in a position to deal with any observations they may make, in such a manner as to obtain certain results. Unfortunately, in the numerous pharmacological and balneological papers which, in addition to those cited above, I examined, hardly a tithe of the results published can be looked upon as certain. It will, therefore, give me much satisfaction, if this contribution of mine should attain its object, and should be of some use in developing the important branch of medical science, of which investigations of this description are the basis.

NOTES ON MEDICAL STATISTICS.

By PROFESSOR CARL VIERORDT, of Tübingen.

I take the liberty of supplementing the preceding[1] paper by a few observations. Four years ago, an able article on statistics, treated especially in a practically medical point of view, was published in this journal by Dr. Schweig; we are now favoured by Professor Radicke with a still more valuable contribution, in which numerous theoretical questions, of primary importance, are also discussed in a way which makes them easily intelligible to the medical public. This is so much the more a subject of congratulation, since the works in this department of medical investigation (notwithstanding all that has been written on method, &c.) are not only very rare, but also (I refer here, especially, to the very valuable 'Principles of Medical Statistics,' published fifteen years ago, by Gavarret), unfortunately, attract very little attention, in consequence of their being so foreign in their nature to the ordinary habits of thought of the majority of medical men. The 'Archives' has, therefore, done good service in drawing more attention to so neglected a subject. When once a conviction of the high importance of these questions (whose truly and pre-eminently fundamental character will be recognised so soon as medicine strenuously resolves not to devote herself so exclusively to the convenient path of the more descriptive methods of observing disease) infuses itself amongst the more zealous portion of the medical profession, the numerous, too often distorted, and either immoderately laudatory or unreasonably depreciatory opinions on medical and therapeutical statistics which the public is compelled to adopt, unfortunately, through its ignorance of the principles and possible correlations of medicine and statistics, on the faith of otherwise able medical men, will come spontaneously to an end.

There are three methods of employing medical statistics. The first, and that which is most generally used by practical men, arrives at its numerical results without specially indicating the magnitude

[1] This paper of Professor Vierordt immediately follows that of Professor Radicke, in the 'Archiv für Physiologische Heilkunde.'—*Translator.*

of the errors in its empirically obtained numbers. All that it recognises, as a rule, is the fact that a statistical deduction which is founded on a broad basis is of more value than one which rests only on a few isolated instances. The second method contemplates the numbers obtained from the stand-point of the calculus of probabilities. Its judgment as to the limits of error in the numerical results obtained, and, consequently, on the value of the conclusions which are drawn from the observations, can only be a *purely formal one.* This formal criterion is, however, in the highest degree a valuable and important one.

The third method subjects the result that is empirically obtained to the same kind of mathematical treatment;[1] but it also recognises the fact, that, in addition to the purely formal consideration of the numbers of any investigation, there may be other and independent reasons, arising from *the nature of the object itself,* which may, and even must be, regarded in drawing a conclusion. Such "intrinsic reasons" for the admissibility or non-admissibility of a conclusion from numbers empirically obtained, are frequently altogether unknown; or, if the investigator wishes to throw off all prejudice, they must be ignored (e. g. in the case of nearly all therapeutic questions, especially those of a very complicated nature), and we are then exclusively dependent on the *formal criticism* of the result. These reasons are, in many cases, obvious only to the penetrating intuition of an adept; whilst in others they lie on the surface, and may be recognised by any one. It is natural that these reasons should be endlessly varied, according to the nature of the object of investigation itself; they are, too, not easily amenable to dogmatic exposition; at least, I can unhesitatingly assert, that up to the present time no one has investigated the correlation of the individual doctrines of medicine with formal statistics; not even excepting the valuable details supplied by Gavarret, in the work previously mentioned. The case of these intrinsic reasons, too, is very different from that of those with which the mathematical theory of statistics supplies us; for these latter are based upon more or less definite—we may even say, for many medical applications,[2] upon nearly abso-

[1] Of course it will be self-evident, that the actual employment of these mathematical operations may be dispensed with by those who are sufficiently adepts to recognise at a glance the correct data for a conclusion.

[2] I say, "upon more or less definite rules," since all the formulæ of the calculus of probabilities (I refer here only to the so-called indirect calculus)

lute rules. By far the most important circumstance in reference to the criticism of these "intrinsic" reasons, in opposition to the importance of the purely formal criticism of the numerical results of investigation, is the one that the former are not amenable to numerical expression. We can at the most attain only to an incidental appreciation of their importance from their number, and the value which we affix to each of them individually.

Let us now examine some of their most important mutual relations. In the first place, then, with a formally accurate result, such as will stand the severe test of the calculus of probabilities, are associated a number of reasons that are both relevant and free from the instigation of prejudice. These latter may (though they do not necessarily) increase the certainty of the conclusion, i. e. the limit of error may be narrower than the mathematical theory, based upon the number of the individual observations and the criticism of the ultimate Mean obtained, requires. In the second place, a numerical result that is formally deficient may be, to a certain extent, propped up, and, within certain limits corroborated, by means of intrinsic reasons; i. e. the expert may have grounds, under certain circumstances, to accept a result which an investigator, who proceeded upon simply theoretical and mathematical considerations, would be obliged to reject. And, thirdly, a *formally accurate* numerical result may be not, on that account, a whit the less incorrect (how often has this occurred in the experimental sciences!), and may, under certain circumstances, be at once recognised as false by the expert, on intrinsic grounds alone. In this last case, I, of course, assume that the mere mechanism of investigation is irreproachable.

The foregoing remarks can involve me in no charge of misconception; least of all, that I ignore the importance of the calculus of probabilities, whose leading principles and special applications to medicine I expressly explain each year to my class. Even in the comparatively simple questions which we address to the organism I am in the habit of demanding the widest possible basis of facts for

must proceed from a definite numerically expressed probability. Thus, for example, Poisson bases his formulæ upon a probability of (in round numbers) 200 to 1. The beginner who has not, as yet, reflected upon the theoretical and practical importance of the definition of certainty and probability, need not be surprised at this: such a probability may almost be assumed as a certainty in the case of most medical questions. What credit should we not give a man if he were only to make one false prognosis in 200 cases of typhus fever!

supporting a conclusion; how much more so in those almost uniformly complex problems which we are called upon, in therapeutics, to solve. Yet I do this at the same time with the reservation that, in addition to the purely formal and mathematically convincing logic of the calculus of probabilities, there is *in many cases* a logic of facts, which, when applied in the proper manner and in the proper place (i. e. to questions that are not too complex), carries with it, for the man who is acquainted with his subject, a small, or, it may be, a very high degree of conviction. I will endeavour specially to apply what has been said above to the criticisms which Professor Radicke has emitted with reference to certain conclusions which Kaupp has drawn from his series of investigations on the urine, instituted in the physiological laboratory here.

In the first place we must, for the sake of fairness, remember (what the critic omits to mention), viz., that the researches in question are entitled "Investigations on the dependence of the amount of chloride of sodium in the urine upon that in the food." The *direct* and most important results of these investigations are, consequently, not in the least affected by Professor Radicke's criticisms. They are open to no objection, and have been recognised by the highest authorities.[1] The lengthy and obliging criticisms with which the writer honours the production of my young friend, refer only to two accessory effects of the saline diet (viz., on the urea, and on the total amount of the urine), and to the dependence of the quantity of the urine upon the temperature of the atmosphere. Now, when an investigator pursues a manner of life so uniform during a series of researches lasting over so many weeks as Kaupp did (I do not think it is everybody who would readily bring himself to undertake a similarly protracted labour, during the most monotonous way of living possible), such a series would appear to offer a guarantee for the institution of a series of allied researches. A critic who would wish us to refrain from availing ourselves of labours bearing so wide an application would not be worth answering. The defence which I shall offer of these conclusions of Kaupp, which have been attacked by

[1] Kaupp's figures in reference to the relations of the amount of chloride of sodium in the urine and food, might have been quoted by Professor Radicke as at least as good an example of a series of researches instituted on the organism under influences that were considerably divergent, both in their action and measurements, as the researches of Boecker, which hence receive the sanction of a representative of the exact sciences.

Professor Radicke, will stand or fall with the correctness of the two following theses, which I believe to be not unworthy of consideration, viz.:

(1.) We will suppose that an effect (such as that of a diminution in the amount of urine from a rise of the temperature of the air) may be assumed as *à priori* certain upon irrefragable physiological grounds, so that any researches upon questions connected with it are only instituted with the view of ascertaining, so far as possible, the *magnitude* of the effect which the agency in question exerts. For this purpose researches are instituted, and numerical values obtained, whose ultimate Means substantiate the assumed influence. Is, now, the result of these researches entirely unworthy of confidence if the differences of the numbers thus empirically obtained still lie within the limits which the calculus of probabilities defines as those within which (irrespective of other considerations) no conclusion as to the influence of the supposed agency can be properly admitted? Certainly not! For, to supplement the formal deficiency of the researches, grounds of conviction from the nature of the case itself come into operation which are not amenable to the logic of the calculus of probabilities. If this assumption is allowed, the objections which Radicke makes to the probability of Kaupp's assertions as to the influence of heat on the amount of urine lose a portion of their weight.[1]

(2.) A single series of observations on one individual leads to a certain number as an ultimate Mean; this latter labours under the formal error just referred to in (1). But in this case there are no, or at least only the slightest intrinsic reasons upon which to base a material criticism of the result. Suppose, now, that a second series of observations on the same question is instituted on another individual; and that it gives analogous results, and is open to the same errors. Any conclusion deduced from a *single* series is, so far as it alone goes, only to be admitted with caution; but when

[1] At the conclusion of his paper, Professor Radicke indicates the researches in question as "only favouring, but not proving," the assumption of the influence of heat on the amount of the urine. This unexpected admission brings him as near as possible to the view which I myself take of the matter. Kaupp pursued two paths for the purpose of statistically testing the influence of temperature; and as he spontaneously abandons one and follows up the other (which Professor Radicke asserts is the better of the two), he obviously does not deserve the blame of having exclusively pursued the former of these. In questions of this kind, even the least promising and justifiable paths ought not to be altogether neglected.

two series agree with one another in their results, we have therein a guarantee, to a certain extent, that these results are worthy of our attention. The amount of increased certainty, which result A acquires by the concordance with it of result B, cannot, it is true, be closely estimated. Still it is obvious that if A gives the same result from forty observations as B does from sixty, the importance of that result will be very different from what it would be if it were based upon one hundred, or two hundred, or even a greater number of observations on A alone. For in the case of observations instituted upon A alone, the result might possibly be considerably affected by individual or external influences which might come into operation during the period of inquiry. But the fact of the results obtained from B being more or less analogous to those from A, gets rid of a certain amount of doubt on this score, always, of course, assuming that we have to deal with a *comparatively simple* question. By this concordance, the limits of error in result A must obviously be to some extent diminished. The calculus of probabilities, however, would disclose nothing to us on this head, or at least it could not do so with any certainty until after several series of researches on different individuals, which in the case of by far the greater number of delicate protracted inquiries on the organism would be as impracticable, as in comparatively simple ones it, fortunately, is superfluous.

A REPLY TO PROFESSOR RADICKE'S PAPER,

"ON THE IMPORTANCE AND VALUE OF ARITHMETIC MEANS."

By Dr. F. W. BENEKE, of Marburg.[1]

(From the 'Archiv für Physiologische Heilkunde,' Part IV, 1858.)

In Professor Radicke's paper on the "Importance and Value of Arithmetic Means," the following passage occurs: "Consequently, the remark which Dr. Boecker, of Bonn, made, when he gave me Dr. Beneke's paper (on the Influence of Bathing in the North Sea, Göttingen, 1854),[2] 'that not one of the results contained in it appeared to him well founded—at least so far as the part containing the researches on the urine were concerned'—is fully confirmed." This opinion of a professed mathematician, which is expressed in such decided language, affects so severely, and may possibly convey to many readers so unfavorable an impression of my work, that I am compelled to publish the following reply and protest; and, in so doing, it is not merely a defence of my unpretending contribution that I propose to offer, but to discuss a question of general importance in relation to many of our undertakings in the present day. And I may the more appropriately offer the following remarks for the consideration of my fellow-labourers, since I myself take a peculiar interest in the class of undertakings to which I refer, and have for some time been engaged in them.

I shall, in the first place, direct attention to the *general* part of Professor Radicke's lengthy paper.

The author states that, at the request of his friend Dr. Boecker,

[1] See 'Archiv für Physiologische Heilkunde,' 1857, p. 501.

[2] As the amount of space at command precluded the insertion of this paper in its entirety, such portions only have been extracted as refer immediately to the *general question* at issue; the *special defence*, therefore, which Dr. Beneke offers of his original investigations, is necessarily excluded. It has also been found requisite to condense slightly some of the passages quoted.—*Translator.*

he has undertaken, "as one who is acquainted with mathematics and physics, to provide medical men with a ready test by which they may try the correctness of the conclusions at which they may arrive," and to help Dr. Boecker "to stem the stream of baseless and comparatively erroneous doctrines, by which medical science is daily threatened to be overwhelmed." The defect in all processes which have been hitherto employed by us in investigations respecting the metamorphosis of tissue, he states to consist in the want of a clear perception of the import and value of Arithmetic Means, and, as a consequence, in attributing to them an importance which, in applications of this kind, they do not really possess. With reference to the preliminary remarks of our author, I must, at the outset, call in question the competency of mathematicians and physicists as authoritative critics of medical and physiologico-chemical matters, unless they are themselves to a certain extent physiologists. For my own part, I should wish for nothing more than that we might succeed in defining, with an accuracy which might deserve the epithet mathematical, the various phenomena of life, with their numerous and interesting fluctuations, and that we might exchange our ever-fluctuating *ideas* of them for precise and faultless numerical expressions. I believe that more may be accomplished in this direction than has hitherto been done, and that the numbers obtained in researches, and even the researches themselves, have been very inconsiderately employed; and I consider the trouble which Professor Radicke has taken in directing attention anew to this point, and in showing a way to remedy these defects, as worthy of all praise; for if our medical and physiological statistics are to have any real value, they must necessarily be founded upon numbers which are free from avoidable errors. But although, in this point of view, we readily and with good reason allow a certain importance to the mathematical estimation and examination of the numbers obtained in our investigations, yet I must maintain a sound physiological discrimination to be superior to this, and on this account I entirely agree with the remark which Professor Vierordt makes in answer to Professor Radicke, "that, in addition to the purely formal and mathematically demonstrable logic of the calculus of probabilities, there is, *in many cases*, in addition, a logic of the facts themselves, which, when employed in a proper manner and place (i.e., in questions that are not too complex), carries with it, for the man who is acquainted with his subject, it may be a small, or it may be a high, degree of conviction."

We can estimate accurately enough the working capacity of a steam engine under given conditions, and what change will be effected in it by an alteration of those conditions. On this point we obtain infallible numbers, in determining the value of which we have no further difficulty.

But the animal, or, at least, the human organism, is not such an engine. Day by day, and hour by hour, influences are at work upon it which may alter the course of the vital processes to a greater or less extent; and whatever may be the nature of the investigations which we institute upon it, it will always be more necessary accurately to estimate these influences, i. e., to test the experiment physiologically, than to employ the keen analysis of mathematics upon the numbers we obtain. It is, I think, a mistake to expect to make the results which have been hitherto obtained by physiologico-chemical experiments *more certain* by mathematical examination of the series of numbers obtained. Every one, on the other hand, who has carried out many investigations of this kind, knows that during the progress of his experiments a confidence or mistrust in the results arises, which is much more convincing than the numbers themselves, and the experimenter is often without doubt as to the correctness of his result, notwithstanding the demonstration of the mathematician that the difference which may have been found between the two Means obtained from the series under examination, lies within the limits of error of the calculation. It is possible—and I wish and hope that it may be so—that the advice of Professor Radicke may be the means of communicating to our results a greater accuracy than they have hitherto often had, and that they may obviate hasty and decided generalizations. But I also think that the physiologist, as well as the medical man, will look upon all the numerical values connected with these investigations as only approximately correct, and that the most careful estimate of probabilities will not rectify the numbers, if proper physiological considerations have been deficient, or if careful manipulation and processes were not adopted in obtaining them. The valuation of the numbers—the determination of their Arithmetic Mean—must also always precede the examination of them which the experimenter should make; and it is requisite that the experimenter, as well as everyone who attempts to criticise them, should possess sufficient acquaintance with physiology, in order to estimate them at their absolute value, as well as to test the greater or lesser probability of accuracy which they possess. The

mathematician, as such, cannot do this, and however accurately he may determine the Mean of a given series of numbers, he works with quantities whose value he himself cannot estimate; and he may in the end evolve a result as mathematically correct, which is probably altogether false, in consequence of its being founded upon individual observations which are faulty.

Professor Radicke provides us with a convenient illustration of the views which I have just expressed. The investigations of Dr. Boecker on the influence of a saccharine diet upon the metamorphosis of tissue, receive in Radicke's paper, as Vierordt remarks, "the sanction of a representative of the exact sciences." Boecker arrived at the conclusion that the ingestion of sugar decidedly retarded the metamorphosis of tissue, and, amongst other effects, also diminished considerably the diurnal excretion of urea. According to the calculations of Professor Radicke, on the days when sugar was taken 15.7 gram. of urea, with the mean fluctuation of 2.03 gram., and on the days when no sugar was taken on an average 24.5 gram., with the mean fluctuation of 4.76, were eliminated. Professor Radicke appends to this conclusion the remark, "the difference of the Means is 8.8, and the sum of the mean fluctuations 6.79; so that we cannot recognise a decided diminution of the urea under the influence of the saccharine diet." But will the doubts of the physiologist be satisfied with this expression of opinion, and will it be considered as definitive? For my own part I have very great doubt whether, by the ingestion of sugar (with the continuance, be it observed, of the usual dietary and method of living, as in Boecker's investigations), the amount of urea in the urine can be diminished to $\frac{2}{3}$ds of its normal quantity; and although the conclusion to which Dr. Boecker comes as to the retardation of tissue metamorphosis may generally approach the truth, the numbers which he has obtained appear to me to be very doubtful, and to demand further confirmation. The numbers 13, 14, 15, &c. grammes of urea *per diem* for a man of about 136 lbs. weight possess more improbability for the physiologist, under the circumstances in question, than a mean number, with whose determination the mathematician is not, perhaps, quite satisfied, but which is, at least, deduced from numbers that are not improbable; hence I feel compelled in this case to consider the opinion of the mathematician as inferior to that of the physiologist.

Accordingly, I think that I am justified in demanding, that the first

opinion as to the accuracy and reliability of physiologico-chemical investigations, of which alone I am here speaking, should continue to emanate from the physiologist or physician, who know how to estimate how far the way in which the investigation is conducted, the numbers obtained, the conclusions drawn, and, still more, the ruling ideas and general cast of mind of the experimenter, are deserving of confidence, and correspond to our previous knowledge. After this the mathematician may be readily allowed to intervene with his opinion as to the calculation of the individual numbers, and especially of the Means which have been obtained. In all cases he will be able, except where he may discover great errors in the calculations, to give an opinion as to greater or lesser amount of probability in the individual conclusions of the physiologist, and thereby contribute to the acquisition of truth, always provided, however, that he takes into careful consideration the grounds upon which the physiologist bases those conclusions.

In what way does Professor Radicke propose to increase the certainty of our conclusions? After he has treated of Means generally, and has explained to us very clearly what are the distinctions between arithmetic, geometric, harmonic, and quadratic Means, he proceeds to the special examination of Arithmetic Means as being those which are most of interest to us. An Arithmetic Mean may have a threefold meaning. It may either exhibit a pure average; or it indicates the probable value of a definite fixed quantity; or, lastly, the probable value of a variable quantity taken under mean conditions. In the first of these cases we have to deal with *numerous* numbers of *definite* value; with numbers which are independent of one another, or of whose dependence no notice is taken. In the second case, only *one* number is involved, which has to be determined from numerous others of *uncertain* value. In the third case, we have the same conditions as in the second, with this exception, that the number to be determined indicates, not a definite, fixed quantity, but a variable one. Our physiologico-chemical investigations nearly always are of the third class; hence it is requisite accurately to appreciate the degree of uncertainty or fluctuation of the individual numbers obtained, and to estimate the probable value of the Mean with reference to that degree of uncertainty. For this purpose Professor Radicke gives the following rules as applicable: "the Mean required is obtained by taking the certain figures which a consideration of the successive Means presents, and by adding to them as the succeeding figure the

Arithmetic Mean of the first uncertain figures, taken from an aggregate of so many of the last successive Means as the given case shows to be desirable. The greatest divergence in the numbers of this aggregate from the Mean found, is *the measure of uncertainty.*" If we have to compare the Means of numerous complete series of observations of variable quantities, we are then only justified in concluding that an increase has occurred on one side if the difference of the Means is greater than the double of the greatest error of observation, i. e., *if it is greater than the sum of the uncertainties of these Means.*

With regard to these recommendations, the computation of the successive Means, as well as the determination of the measure of uncertainty, appears to me very desirable; and, as Radicke advises, the successive Means of the numerical series ought to be taken both from first to last, and from last to first, so as to discover whether the uncertainty is greatest at the commencement or the termination of the series. By means of these processes we shall certainly gain a more correct idea of the fluctuations of the results of our investigations than we should by a simple survey of them, and it is only in this way that we can acquire a knowledge of the degree of mathematical accuracy which the Mean possesses. With respect, however, to the second of the above conditions, I can only accept it as partially true, however correct it may be in its mathematical relations. For, the sum of the uncertainties of certain numerical series will often exceed the difference of the mean values of these series, without that difference being on that account the less an approximate expression of the truth. Let me illustrate this by the following example:

Professor Radicke is favorable, as has been already mentioned, to Boecker's conclusion, that a saccharine diet diminishes the amount of urea in the urine, because the difference of Means in the two series compared = 8.8 gram., exceeds the sum of their mean fluctuations = 6.79 gram. The sum of the fluctuations of two series in respect to urea will probably often, and upon physiological grounds, exceed the maximum of 6.79 gram. For there are numerous influences, medicinal agents, and articles of diet, which diminish the quantity of urea by $\frac{1}{5}$th of its normal amount; but shall we, after repeated and frequent discovery of slighter differences than this, conclude that those influences, &c., have no effect upon the urea, simply because the sum of the uncertainties of the numbers which our investigations give is less than the difference of their Means? Or, to take another example: Professor Radicke estimates

that the uncertainties of the quantities of urine obtained by Boecker in two series of researches, of twelve days each, with reference to the action of sarsaparilla, amount to 386 cub. cent., and 268 cub. cent., or, in the aggregate, to 654 cub. cent. (not 674), whilst the Means of the two series, are 1549 and 1499 cub. cent. Uncertainties of an entirely similar character may be found in the investigations of others, as, for instance, in my own, published in the 'Archiv des Vereins für Gemenischaftliche Arberten.' Yet these Means are very like those given for the urinary quantities during an ordinary course of life by other investigators. But, supposing now that we find in another series of researches the same uncertainties as in Boecker's, but with a difference of 400 cub. cent. in the Means, Professor Radicke will by no means allow that the influence under investigation has produced any alteration in the urine, because the difference of the Means is smaller than the sum of their uncertainties = 674 cub. cent., or to about half the normal quantity of the urine. Will any physiologist doubt that an average diminution of 400 cub. cent., or 1500 cub. cent., as the normal amount of the urine, as a sequence of the action of any given influence, indicates that that influence has produced this diminution of the urine, provided that this latter result has not been excited by some special *accidental* condition, such as sweating, diarrhœa, &c.? I may in fact assume, that the sum of the uncertainties of two series of investigations is very frequently greater than the difference of their Means, especially when the influence to be tested is not a very powerful one, such as the deprivation of water or meat; yet if we are to recognise such investigations as justificatory of a conclusion only when they correspond with the conditions Professor Radicke has laid down, we should probably seldom come to any conclusion at all. We must in discussing these matters, bear in mind the important fact, that the fluctuations in the vital phenomena compensate one another within a certain number of days, provided that no excessive disturbing causes have intervened. Thus, for instance, on one day, as a result of severe mental pressure, we may have 1900 cub. cent. of urine excreted, whilst on the following day (equal quantities of fluid being consumed on both days) the quantity excreted may be only 800 cub. cent.; yet in four days, notwithstanding this fluctuation, the quantity excreted will be nearly the same as if this disturbance had not occurred, or as it would be on four other days in which equal quantities of fluid were consumed, but all the other conditions under which the organism

was placed were unchanged. Under such circumstances as these, a physiologist will place no reliance upon small differences between the Means, nor will the mathematical determination of the uncertainty restrain him from coming to a conclusion, although the difference between the Means of two series may be less than that uncertainty. For these reasons, therefore, it appears to me unjustifiable to eliminate numbers that differ considerably from one another, as Radieke does, for no other reason than because of special disturbances; for, in the first place, such numbers bear a certain relation to those which immediately follow them, and these latter without them lose their real value; and in the second, every physiologist will know how to estimate an unusual disturbance, and to value accordingly the numbers which it gives. If the disturbance is so great that the results of the investigation are of necessity affected by it, then it is not only the day on which it occurs, but also the whole series which is unavailable.

The relations in question are in no way affected by the protraction of the series of observations. I may even, on the other hand, assert that by so doing the uncertainty becomes greater, since experience has at least taught me this, that even if the long-continued and unrestricted pursuance of a monotonous series of researches and mode of life does not absolutely disable the organism, it may, and practically does, alter its normal conditions. If a man pursues an investigation on the operation of any given agency upon the metamorphosis of tissue for six or eight days, and then calculates the mean value, that mean value will, as Professor Radicke has clearly exemplified, be always altered if to those six or eight days one or two more are added. How, then, are we to attain a certainty in respect of the probable value of the numbers of which we are in search? It appears to me that such certainty as the result of any one single investigation is impossible, and that neither the length of time during which the investigation is carried on, nor the mathematical determination of the mean values, uncertainties, &c., can give it. There is only one way of attaining to it, and this, so far as my experience goes, consists *in repeating the observations as frequently as possible on different individuals*. All mean values obtained from a single series of observations on one individual, however long it may be, have a certain amount of doubt about them, whether the difference between two given series exceeds the uncertainty or not. The individual organism always exists under varying external and general influences;

very rarely is its condition at all uniform. It is only the frequent repetition of the same observations of one's-self, or by other experimenters on themselves, which can confer upon the results obtained any certainty; and that this view of the question has always been a leading one with me is shown by the demand which I have for many years past made for *co-operative* labours in physiologico-chemical questions on a definite plan. Is Dr. Boecker, then, who entirely coincides with Professor Radicke, of the opinion that his investigations will acquire much more reliability and certainty if he determines his mean values according to strictly mathematical rules, and especially if he says "No," where the difference between the Means of two series does not exceed the sum of their uncertainties? I need only refer again to the sugar researches quoted above, which the mathematician sanctions, but over which the physiologist will require to think further, to lead me to think that we are flooded with "the stream of baseless and erroneous doctrines by which medical science daily threatens to be overwhelmed," not so much through the keenness with which any one man throws himself into the defence of his own investigations, but because such minimal contributions as these are so excessively lauded by others, especially at their commencement, and are rated so much more highly than they either ought or seek to be. But let no one be very much astonished at this. It is just in science as in common life, the opening of a new source of acquisition (in this case a mental one) exercises a deluding fascination, and readily excites an unjustifiable confidence. This, in time, disappears of its own accord. Truth makes its own way, errors disappear, and then, for the first time, the real value of this new source is rightly appreciated.

I may be allowed, myself, to make a few remarks upon an erroneous apprehension of the mean values heretofore obtained, which is occasionally met with. As a rule, the method which is employed is, to conduct a definite series of investigations in as uniform a manner as possible for a certain number of days; to add together the numbers obtained for the substance, &c., experimented on; to divide the sum by the number of the days; and to regard the quotient as the mean value for twenty-four hours. But these twenty-four hours are no definite ones; nor do they indicate any day that we may like to choose. They have only a comparative value, and all that we are justified in saying with regard to them is, the Mean found within seven days of the month of June, &c., for the period of twenty-four

hours, is of such or such an amount. In reference to this point, it should be remembered this amount is only estimated for the sake of convenience, and that we may have small numbers to deal with, whilst the series of observations compared embrace unequal numbers of days, and that, therefore, the result of the investigation lies strictly in the sums of the individual days. Where the numbers of days in the different series is uniform, it is preferable to adopt the sums of these days, instead of the value determined for twenty-four hours. According to this way of looking at the question each Mean, in our collected observations will have only an approximative value; and it appears to me, for these reasons, that those are in error who set out with the idea of drawing infallible conclusions from one, or even several, mean numbers.

How little good is to be got from a single set of observations on one individual appears very clearly from Professor Radicke's own considerations. In the first place, he shows, in two investigations of Boecker's, that the mean quantities of urine, viz., 1499 and 1549 would have become converted into 1554 and 1494, if the observations had been limited to eight days instead of twelve; and that the Means would have again changed to 1545 and 1515, if the thirteenth day had exhibited the same quantity as the twelfth. In another place, Professor Radicke rejects a conclusion of mine, which I certainly never drew, viz., that sea-bathing diminishes the urinary secretion. He states, that if only the first four days of my seven days' investigation on the action of sea-bathing are considered, and the data of these days compared with those of the four days without bathing, which appear to him equally objectionable, no diminution of the urine, but rather an increase (about 18 cub. cent.) will be evident. This is correct: but if the last four days of the seven days' series are considered, and compared with the preceding four days, a very evident diminution of the urine will be seen; and were it not that my doing so might justify the assertion which has been fathered upon me, I would cite this circumstance as a proof that the fluctuations of the numbers in a single series of observations, and on a single individual, *allow of no certain conclusion being drawn from them*, although they may give impressions of a highly approximative accuracy—impressions whose certainty is not much increased by a mathematical estimation of the value of the numbers obtained.

Professor Radicke rejects researches of three and four days altogether. As a rule, I agree with him on this point, and I would, espe-

cially in researches of the kind now under consideration, indicate an investigation of at least six days as desirable, but not of any greater length, and, certainly, not so when the experiments are made on one's own person; in which case the sources of error above referred to arise. But, in special cases, and under certain circumstances, I think that we may rest satisfied with investigations of four days. Thus, when Professor Radicke speaks of certain mean values obtained from four days' observations in my researches on bathing in the North Sea, as entirely inadmissible, he would have been quite right if the numbers I obtained from these four days' observations had been the first which I had obtained from researches on my own person. I had, however, already instituted a considerable number of similar observations on my own body, and am always in a position to decide, after four days' observation on a new subject, whether notable or real changes have occurred in my organism. Whether this fact will justify conclusions being credited from me which one would hardly admit from a tyro, I must leave to the judgment of others. It goes, however, as hardly with the labours of others as with my own, before Professor Radicke's seat of judgment, and Kaupp receives a reprimand which Professor Vierordt at once dismisses as little deserved. Are we, then, in each of our small papers, or monographs, in each individual undertaking, to make an exposition anew of our general knowledge on the subject under investigation; and are we, above all, to append to each number and expression the whole cargo of references which can by any means be dragged in with it? In scientific matters, in addition to mathematical proofs of a fact, there is a certain amount of confidence exhibited towards the person who affirms the fact: if this latter be wanting, all numbers which may be adduced in support of physiologico-chemical facts will appear questionable, and all the mathematical corroboration which may be given to them will not make them less so: if it be present, we shall credit the investigator with having exercised due supervision over his numbers, and the days on which his observations have been made, before he founded an opinion or hypothesis upon them. The numbers in which the results of our researches are expressed, moreover, carry with them, intrinsically, more or less of our confidence, in proportion as they agree or not with general physiological or medical experience. If such an agreement be wanting, both experimenter and critic will alike, and with justice, feel great doubt with regard to the results obtained; they will be still more desirous of repeating the experiments; and not even the

most exact mathematical estimates of the numerical series involved will give them a feeling of certainty. If, on the other hand, the harmony with well-known physiological facts is evident, there will be less doubt; and, however much further examination may be desirable, the result obtained will, nevertheless, on independent grounds, deserve consideration, as throwing light upon the question under investigation. What we seek in all our chemico-physiological investigations is not the whole truth; all that they can do is to give us a nearer acquaintance with certain principles, and a nearer insight into the chain of vital phenomena under varying conditions.

I have thus generally mentioned the principal objections which I feel compelled to offer to Professor Radicke's recommendations, but shall refrain from any more minute discussion of individual points. I repeat, that I consider a precise and mathematical examination of the numbers of such chemico-physiological investigations as we are at present discussing, as well as a careful valuation of the individual numerical series as most desirable; I thankfully recognise the instructiveness of Professor Radicke's labours in several points, and have not perused them without advantage; nevertheless, I maintain that the physiological tact and knowledge of the experimenter appears to me as of more importance than the mathematical; that it is only the *greatest possible repetition* of the observations on different persons and under different circumstances, that can give any positiveness to our results; and I cannot certainly consider it a misfortune that we provisionally retain our older method of determining mean values so long as we do not place too much reliance upon single results, recognise the amount of uncertainty generally existing, and make the number of our observations long and decided enough both for the question under investigation, and for the experimenter himself.

ON THE DEDUCTION OF PHYSIOLOGICAL AND PHARMACO-DYNAMICAL PROBABILITIES FROM CO-ORDINATED SERIES OF OBSERVATIONS.

By Professor RADICKE, of Bonn;

BEING A REPLY TO CRITICISMS ON THE AUTHOR'S PAPER ON

"ARITHMETIC MEANS,"

In Roser's 'Archiv für Heilkunde,' which are based upon a misunderstanding of the same.[1]

My paper on Arithmetic Means has been attacked by several medical statisticians. It might have been naturally presumed that men who had undertaken very laborious and tedious investigations, would look on most impatiently when the validity of the results of their labours was called in question, especially when the doubt so expressed was founded upon mathematical considerations. I believe, however, that the motive to their opposition lay less in dissatisfaction on this score, than in surprise at finding the accuracy of their conclusions denied, on the faith of the rules laid down by me, notwithstanding that the observations of one of them comprised a series of numbers of inordinate magnitude.[2] Less umbrage would have been taken on this account if the real meaning of these rules had been more completely perceived than it has been. From a shyness of mathematical deductions, the rules themselves have not been attacked, but an attempt has been made to limit the sphere of their application, and especially to repudiate their applicability to medical investigations. And with the view of supporting this protest, as well as to elucidate the antithesis that is pretended to exist between the (so-called) phenomenal and the mathematical result of an inves-

[1] In the case of Dr. Beneke, this feeling of discontent appears to have played a very prominent part, since he has committed himself in his answer to erroneous and groundless accusations, which I have met in a reply forwarded to 'Roser's Journal' a short time ago, the publication of which will soon take place.

(This reply has not yet been published.)—*Translator.*

[2] Omitted from 'Moleschott's Untersuchungen zur Naturlehre des Menschen und der Thiere,' vol. vi, part 4, 1859.

tigation, it was at one time assumed that, in order to lay down a mathematical rule on the matters in question, a certain amount of physiological knowledge was prerequisite; and at another, that in addition to the logic upon which the superstructure of mathematics rests, there is yet another kind (which people have chosen to call the *logic of facts*).

With regard to the first of these objections, no one has ever taken the trouble to ask whether I possess a competent acquaintance with physiology, or not;[1] though it is just as likely, that as we have at the present time physiologists who possess at the same time a skilled acquaintance with mathematics, there may be also mathematicians who are equally well acquainted with physiology. But this has nothing at all to do with the question. If it had, we might, with equal justice, question the general application of the rule of three, on the ground that those who employ it should, at the same time, be skilled in physics, chemistry, arboriculture (Forstwissenschaft),[2] commerce and finance, as well as in the thousand other matters in which it is used, which would be impossible. The fact is, that all that it is necessary to know, in laying down a rule for the comparison of two series of observations is—what we may learn from the series themselves, viz., whether fluctuations exist in those series, and if so, how great those fluctuations are. An acquaintance with their causes is quite unimportant, for in that case it is not sufficient accurately to map out these causes, he must know also the laws of their physiological operation. Up to the present time, however, nothing certain is known with regard to the laws which govern any of the unavoidable causes of fluctuation, not even the operation of temperature, notwithstanding the interesting series of observations of Kaupp on that subject, which extend over eighty-seven days. And even if the exact law which regulates any one of these causes of fluctuation were known, its proper consideration would be within the province of mathematics. Dr. Lehmann, therefore, is in error when he maintains,[3] that the irregular operation of unknown causes

[1] The fact is, that I have in my leisure hours occupied myself much with physiological questions.

[2] This branch of applied science, of which our only representative is the governmental department of Woods and Forests, is a recognised profession in Germany, where the absence of coal renders the preservation and culture of existing forests of the highest importance.—*Translator.*

[3] See his paper "On the Estimation of the Physiological Action of Hip Baths." Moleschott's Untersuchungen, vol. vi, p. 186, 1859.

of disturbance is an obstacle to the application of rules which are based upon mathematical considerations. If he had only read my paper upon Arithmetic Means with attention, he would have seen that the consideration of such unknown causes of disturbance plays a leading part in the determination of the rules I have laid down.

With regard to the second piece of information with which we have been favoured, the idea of the existence of two kinds of logic, it is not probable that it will find many supporters. The antagonism which, as Dr. Lehmann and others assert, occasionally appears to exist between mathematics and practical knowledge, will be easily cleared up, if the import of the rules I have laid down is more accurately studied. To make their meaning as evident as possible is the object of the present paper; and I shall avail myself of this opportunity, to provide my opponents with better weapons than they have hitherto used against me, whilst I may even moderate their objections to the mathematical complexion of the rules I have laid down, by showing them that, in their origin, those rules are not purely mathematical, but that they contain a certain arbitrary element. The only effect, however, of this arbitrary element, is to leave room in the limits of rules for a slight fluctuation in the reliability of the result deduced from the Means obtained. That my final opinion neither was intended to be, nor could be, absolutely reliable, but that it admitted of variation within certain limits, was indicated in the first section of my paper. The state of the case, then, is as follows: the number of observations is not large enough in any one of the physiological or pharmaco-dynamical series of researches which have been hitherto published, to enable us to assert that the unavoidable causes of disturbance have compensated one another at the time when their Arithmetic Mean was taken. A certain number of these disturbing causes, therefore, the amount of which cannot be determined, at least not by mathematics, will remain unneutralised. Thus, if of two series of observations, A and B, the first gives a higher Arithmetic Mean than the second, we must still remain ignorant whether the preponderance of one Mean over the other arises from the unneutralised portion of the disturbing causes, or whether it has been conditioned by the agency whose operation we are investigating; hence we can have no absolute certainty as to whether the effect of that agency is to increase or diminish. But although we may not be able to attain to *absolute certainty* on this point, we may, nevertheless, feel that there is a greater or a less

degree of *probability* about it. The greater the divergence of the two Means is from one another, in comparison with the magnitude of the disturbing causes, which may be recognised by the fluctuations of the numbers in any given series of observations—the more probable it is that the effect is to be entirely, or at least partially ascribed to the agency under observation. The question which we have, therefore, to determine, is, what is the *minimum* mean difference in comparison with the fluctuations, which will justify us in concluding, with sufficient certainty, that the greater amount of the Mean in series A is due to the operation of the agency under investigation?

If the observations are sufficiently numerous to allow us to assume that the causes of disturbance have not operated uniformly, or preponderatingly, in the same direction in one of the series to be compared—or that they have not preponderated in their operation in different directions—we may then measure the causes of disturbance by the fluctuations, and assume that the excess of the Mean of series A was caused by the agency *if the mean difference exceeds the greatest fluctuation.* But as this is very rarely the case, and as the test becomes, therefore, of less practical value, it may be asked whether a smaller divergence in the Means, and if so, to what extent, will not allow us to infer with sufficient probability the operation of the agency under investigation in favour of series A. With all our mathematical knowledge, however, it is impossible to answer this question positively, in such a way as to be beyond objection, because it is impossible to determine the real amount of the disturbing causes, and also because one person may place greater reliance in the certainty of the result than another would, which depends upon the undefinable nature of the expression, "sufficient probability." Now if we assume a certain *minimum* as our normal standard, our proceedings will partake (within certain limits, at least) of an arbitrary character. This arbitrary character I gave to my rules when I stated that the mean difference should at least exceed, not the *maximum,* but the *mean fluctuations,* which are sensibly smaller. Another mathematician might probably have been satisfied with the mean difference exceeding *the half of the mean fluctuations.* Dr. Lehmann, for example, is convinced of the operation of the agent, if the mean difference is greater than the mean fluctuation of either of the two series (at least that is what in effect he means). Another

investigator might, on the other hand, have required a still greater preponderance of the mean difference.

It is true that my process is a very stringent one, and that in my rules I require that the amount of the mean difference should be *pretty considerable,* and this is the ground of the so-called *antagonism* of mathematics to practical knowledge which has been referred to above, i. e., the cause of the circumstance that my rules occasionally deny sufficient certainty to the conclusion, notwithstanding that series A exhibits numbers that preponderate greatly over those of series B. However, I must, as far as I myself am concerned, stand by these rules of mine, which are so stringent, and require so much, because I feel the necessity of a *rather high degree of certainty;* because *such large superstructures are erected* on physiological and pharmacological conclusions; and because new theories, or perhaps new methods of treatment, may be founded upon these conclusions, whose effect, both upon science and practice, is restrictive in proportion as those theories are considered to be well founded, whilst it is too easily forgotten that they are based upon data that are only more or less *probable,* rather than *certain.*

If Dr. Lehmann is satisfied with a less amount of security, and feels convinced of the activity of an agent when the mean difference exceeds the mean fluctuation of *only one* of the series compared, no one will contest his right to be so, and we must admit that he has under the circumstances a tolerable degree of probability to guide him in his decision. But, so far as I am concerned, such a degree is not sufficient to enable me to base my conclusions on a certainty proportionate to the importance of the consequences that may be drawn from them.

But Dr. Lehmann lies under a misconception, when he asks whether, "in the cases in which my rules do not admit of any determination it would be better not to possess the numbers which the observations afford, or to avail ourselves of them, so far as their individual value may allow," with the remark that, for his own part, he has no hesitation in deciding in favour of the latter half of the alternative. The meaning of my rules is *not,* as Dr. Lehmann appears to think,[1] that the observations are to be thrown aside if they give no

[1] Dr. Lehmann in several places erroneously expresses himself, to the effect that when my rules admit of no determination, mathematics have nothing to do with the numbers, and that then, other considerations must intervene, to admit of any result being arrived at. It is sufficiently clear, from what has been said

positive answer to the question, whether the agent produces any effect or not, but that, in that case, the potentiality of the agent cannot be considered as absolutely established. In such a case its potentiality will undoubtedly possess a certain amount of probability; and it would be well to catalogue the result under the head of "probable, but as yet not absolutely certain," so as to keep the completion of certainty on the point open until similar results shall have been obtained from later researches.

With the view of meeting the misconception just alluded to, viz., that the observations which do not comply with the conditions of my rules leave the question for the determination of which they were instituted *entirely* unanswered—i. e. that they assert as much *for* as *against* the potentiality of the agent under examination—I have sent to Dr. Boecker a note on the subject, which he has introduced in his paper "On the effects of Fat on the Excretions." (Oesterlen's 'Zeitschrift für Hygieine, Medicinische Statistik und Sanitatspolizei,' 1859, vol. i, part 1, p. 92—5.) In this note I have recommended that in the cases in which the mean difference is less than the sum of the mean fluctuations, but greater than its half, the result which the mean numbers give should be assumed to be "*conditionally reliable*,"

above, that where the conditions of my rules are not complied with, mathematical considerations are by no means dispensed with; and it is expressly stated that, in that case, the conclusion only loses certainty, without, however, on that account, at once sinking below reliable probability (*i. e.*, below the point where mathematical probability becomes less than one half). By "other considerations," Dr. Lehmann means especially the establishment of other indications of reliable probability, which are of a very undefined and precarious character. Thus, for instance, he introduces one of these considerations, where he assumes one of Boecker's double series of eight numbers, on the pretext of making its character more evident, as representing the results of two different classes of business in the transactions of a merchant. The mean difference is 191; the sum of the mean fluctuations is 234; therefore mathematics leave the result undecided, and it is added, "that, so far as mathematical considerations are concerned, the merchant would, by prolonging his course of business, be a loser, though he would feel justified from the numbers in carrying out such a course." If this illustration is to pass muster, the eight numbers in question ought to represent the results obtained on any eight individual days (I might even assert, of any eight individual *weeks*), and I should therefore consider the supposed merchant very rash, if he were to enter upon a new course of business on the faith of eight such numbers, which admit of no conclusion whatever as to how much of the results obtained was caused by simple coincidences. The "*other considerations*" in this case consist only in a *superficial view of the numbers involved.*

though its employment should be suspended until other investigations, undertaken with the view of ascertaining the same point, shall have given concordant results of at least equal reliability.

The limits of conditional reliability here indicated numerically approximate to Lehmann's limits of (complete) reliability, viz., that the mean difference exceed the mean fluctuation of one of the series, in proportion as the two series resemble one another in the degree of their fluctuations; but they differ from them in the fact of taking into consideration the relations, not of one only, but of both the series of observations.

I have above pointed out the existence of an arbitrary element in my rules as to the limits of reliability. In order to indicate generally to those who may not have perused my paper in the 'Archiv für Heilkunde,' the element in them which is not arbitrary (the mathematical), I would mention that the arbitrary choice of the limits I have assumed is based—first, upon considerations which depend upon mathematical conceptions, viz., upon those of the *probable* and *mean* error; and, secondly, upon the principle that we should seek rather to make these limits too wide than too narrow, so as to confer upon the scientific application of the results an amount of certainty proportionate to the importance of the question at issue. Whether the considerations which are adduced in my paper on Arithmetic Means are sufficiently convincing, I must leave to competent readers to judge. Whatever may be the result at which they may arrive, the demands which I maintained in that paper are neither greater nor less than those which I made at its commencement, when I stated that "since an invariably accurate and absolutely reliable standard for estimating the results obtained from observations is impossible, all that I can offer is the standard which I consider a sufficient one, so far as I myself am concerned, and I shall base my requirements upon it."

ON THE

USE OF COLD IN SURGERY.

BY

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ON THE

USE OF COLD IN SURGERY.

The use of cold as a remedy for hyperæmic and inflammatory conditions is not duly appreciated by most physicians and surgeons of the present day. Though there are few medical men who altogether deny the antiphlogistic action of cold, there are still many who consider it a superfluous remedy, or who restrict its use within very narrow limits, and are fearful of the injurious effects which a too extensive application of it might produce.

I am therefore prepared to meet with very decided opposition from many quarters when I venture to express my belief that, of all remedies which are at our command in the treatment of inflammatory processes, cold is the most important, and to assert that without this remedy I would rather not be a surgeon.

Differences of opinion on this subject are, as is well known, not merely modern. If we look back into the history of surgery, we find that by physicians of the most ancient times cold was sometimes esteemed as a powerful antiphlogistic remedy, and sometimes depreciated as ineffective or dangerous. Thus Hippocrates, and after him, Celsus, recommended the application of cold water to recent injuries of the most various kinds; while Galen, on the other hand, used only warm water and warm oil to wounds, and did so, probably, in consequence of experiments he had made on the effects of snow and cold water upon the tissues of the body.

Aëtius, who lived in the middle of the sixth century, praised anew the use of cold water in the treatment of recent injuries, and in some cases even made use of *cold affusion*.

Rhazes, in the beginning of the tenth century, applied water, made eold by snow, to burns, but treated fraetures with moist warmth.

After that, the use of eold seems to have entirely gone out of praetiee, for Marianus Sanetus, and also Blondus, in the sixteenth eentury, recommend pure cold water as a *new* remedy of rapid and certain effieaey in the treatment of even the most serious wounds. Other eelebrated surgeons of the time, and espeeially Ambrose Paré, Fallopius, and Palatius, express themselves to the same effeet. Palatius applied also eontinued eold affusions to severe wounds with the best results.

In the seventeenth century eold water seems to have been but very little used. The remedy was probably too simple for the surgeons of that time. They treated injuries almost entirely with ointments and plasters, whieh were eompounded of the most various and strange ingredients.

In the eighteenth eentury cold came again into favour. Smith, an Englishman, and Samorier, a Frenehman, were the ehief advocates of cold water in the treatment of wounds. The latter had a speeial liking for *continued local water baths*, and has reported three interesting cases which he eured with that treatment exelusively. He regrets that pure cold water was so little used in the treatment of wounds, and blames the public for it, who thought slightingly of so simple a remedy, and would give credit only to sueh medieaments as were expensive and strange, and came from distant eountries.

The French surgeon Chirac treated a severe wound of the hand, which the Duke of Orlcans had received during the siege of Turin, with continued local baths, and made a good eure of it. (Chirac, 'Observations de Chirurgie,' 1742.)

But, nevertheless, this method of treatment again fell into desuetude in France, and it was not before the latter half of the eighteenth century that German army-surgeons reintroduced the use of cold into medical practiee. Among them the greatest credit is due to Smucker, who was first surgeon-general of the Prussian army under Frederick the Great. He made his observations on the effect of eold in the treatment of injuries of the head, during the siege of Sehweidnitz, in the year 1762. In his work, 'Chirurgischen Wahrnehmungen,' 1774, vol i, p. 9, and p. 153, he first reports twelve eases of injuries of the sealp which ended fatally, in spite of most eareful treatment. This induced him to think of a new method

of treatment, and, by a process of reasoning in accordance with the physiological views of his time, he arrived at the conclusion that cold water ought to be tried. This new mode of proceeding he found most effectual in all subsequent cases, so much so that he introduced the practice among the other surgeons of the Prussian army.

Theden, who was also surgeon-general of the same army, speaks, in his 'Neuen Bemerkungen zur Chirurgie,' 1782, of excellent results from the application of cold water in very dangerous injuries and inflammations. He relates, for instance, how, by the application of cold water, he cured himself of a very severe phlegmonous inflammation of his arm, which was caused by pricking his finger in opening an abscess. He observed very favorable results from the remedy in cases of frostbites, of incarcerated hernia, &c., and ventured to treat even erysipelas with cold water. Although, notwithstanding, he goes on to recommend his "gunshot water," he does this only, as he himself remarks, because envy and spite prevented him from employing the simplest remedy in the treatment of wounds. This "gunshot water" of his, however, he used to dilute with great quantities of pure cold water.

In the year 1785 the French army-surgeons, Percy and Lombard, had opportunities of observing the good results which are to be attained from the use of cold in the treatment of severe injuries. Some new guns were being proved at Strasbourg. Several of them burst, and many soldiers were severely wounded. A miller from the Elsass offered to cure them rapidly, provided the treatment were intrusted exclusively to him. The wounded were, in consequence, handed over to him, and the miller kept his promise. He cured his patients rapidly by using merely cold water, to which he added a little alum, and to which he imparted virtue by various magic spells. Lombard, who watched the proceeding, saw, of course, that the rapid cure of these cases was simply due to the cold water, and he himself obtained similar good results from this remedy in a series of cases which, soon afterwards, the bursting of some other guns brought under his treatment. In the following year he published a paper on the local application of cold water in surgical diseases. Percy, in the year 1792, had a further opportunity of observing the favorable results of the cold-water treatment. While at Saarlouis, he witnessed the success obtained by two German army-surgeons by the exclusive application of cold water in many cases of severe

wounds which were under their care. He afterwards recommended strongly this method of treatment in the 'Dictionnaire des Sciences Médicales.'

This remedy has ever since maintained an honorable place in military surgery. Most authorities in that branch of medical science make use of it, and warmly advocate its application in all kinds of severe injuries. (Larrey, Guthrie, Baudens, Stromeyer, &c.) In civil surgery, on the other hand, as well as in medical practice, cold is far from being duly appreciated, and is, consequently, not employed as it deserves to be. Of this I have had ample opportunity of convincing myself, in visiting many hospitals in different countries and in watching the mode of treatment which many physicians pursue in private practice.

Further, in perusing the handbooks and monographs of modern surgery in reference to this point, one recognises at once that many authors are not acquainted with the results which the medical practitioner is able to obtain from the use of cold, rightly applied. Although most of them admit that, in cases of recent wounds, and in the first stage of acute inflammation, cold water or ice is an excellent remedy, they generally caution us against the long-continued application of cold, and advise its discontinuance as soon as suppuration sets in. But some surgeons go even further than this; they reject altogether the use of cold as an antiphlogistic remedy, and believe that it is apt to produce various bad results, as, for instance, gangrene, phlegmonous inflammation, tetanus, inflammation of internal organs, rheumatic affection of joints, &c. Some of the opponents of cold apply, from the very beginning, to recent injuries and inflammations, the opposite agent—warmth; whilst others flatter themselves that they have discovered the only effectual antiphlogistic remedy in firm supports, obtained by strapping, starch-bandages, and so on.

If I now positively assert that, in my own practice, having made very extensive, though deliberate, use of cold in the most various forms of diseases, I have never had occasion to observe any of those bad consequences which are said to be caused by that remedy, and if I repeat here my previous statement, that, according to my own experience, I believe cold to be the most important and most powerful antiphlogistic remedy, I am naturally led to inquire how conflicting opinions concerning this point have arisen—how it is possible that such different views are entertained on comparatively so simple a subject.

The answer to this question seems to me not a very difficult one.

The stronger the curative power of a remedy is, the more important is it that it should be judiciously and rightly employed, and the more dangerous will the consequences be of its injudicious application. All our most powerful medicaments are poisonous, if wrongly administered, but no reasonable practitioner will expunge opium, for instance, from his list of remedies because a too large dose of it may possibly kill a patient. This applies not merely to the remedies which we get from the apothecary's shop, but just as well to those simple agents of nature, the effects of which modern medical art endeavours to turn more and more to use. If cold, then, be one of the most powerful among these, every injudicious application of it must necessarily be dangerous. But we must attribute the bad consequences by which its use is followed in such cases, certainly not to the remedy itself, but to its improper application.

I feel justified in stating that most of the opponents of cold are adverse to it only because they have never used it, or have never seen it used in a pure and judicious manner. A great number of practitioners with whom I have become acquainted date their aversion to cold from the time of their pupilage, and have never since attempted to make experiments of their own on this remedy because they had inherited from their masters the dread of it, as of a two-edged weapon.

In speaking of the application of cold, the expression "cold compresses" is almost universally made use of. This one circumstance alone proves how little conscious people generally are of the effects which are really produced by using cold in that way. For, as we shall hereafter see, that mode of applying it is, of all modes by which cold can be brought to act, the least to the purpose and the most uncertain; and very often, by that method of application, precisely the opposite of what was intended is effected.

But before I enter upon the consideration of the subject, it seems to me necessary to examine what are the effects which are to be expected, and which can be obtained, from the use of cold in the treatment of inflammatory and similar processes, for then only will it be possible to ascertain if, in a given case, we really produce these effects, or if we come short of them.

One of the most important symptoms, and, according to the best modern observations, the most important factor in inflammatory

processes is the increase of temperature in the inflamed part, which is accompanied with an increase of temperature of the whole mass of blood and of the entire body (fever).

We know that this increase of temperature proceeds, on the one hand, from increased textural changes in the inflamed tissues, and on the other hand, and especially, from an increased flow of blood through the blood-vessels of the part.

Now, if we possess a remedy which diminishes the temperature of the inflamed part as well as that of the whole body, and which, at the same time, removes the sources of the abnormal production of heat, I think we may call it, with full propriety, an antiphlogistic remedy; and that we really do possess such a remedy in cold is amply proved by experiments, as well as by observation at the bedside of the sick.

If we dip a warm and turgid hand for a time into cold water, we notice that it soon becomes pale and cold, that its size is decreased, and its sensibility lessened. The cold water has deprived the immersed member of a part of its warmth. If we then take the hand out of the cold water and dry it, we shall have to wait some time before the blood which is circulating in its blood-vessels will have supplied it with warmth enough to equal the temperature of the other hand. A bladder filled with ice will act in the same way on any part of the body with which it has been in contact for a time. The longer the contact lasts the deeper will the effect extend, and the longer will be the time which will be required, after the removal of the bladder, for the circulating blood to raise the temperature of the cooled part to its original standard.

The same effect may be obtained in very different ways, but, whatever method we use in applying cold, its first and immediate action will always be to deprive the part to which it is applied of more warmth than it would lose under ordinary circumstances, whether exposed to the air or covered with cloths or blankets. This deprivation of warmth acts, at the same time, as a strong irritant, and produces, as such, an energetic contraction of the involuntary muscles of the skin and of those of the blood-vessels. The papillæ of the skin become more prominent, and the part grows paler, because less blood circulates in the contracted blood-vessels.[1] If the cause of irritation is soon removed, dilatation of the blood-vessels follows at once, and with it

[1] According to Donders, the circulation of blood is slower also in cooled parts.

an increased supply of blood; and all this occurs to an abnormal degree if inflammation be present. But if the deprivation of warmth be continued for a longer period, not only the supply of blood, but also the local changes in the tissues, become diminished, because the parts acquire, by degrees, so low a temperature, that chemical processes take place in them with increasing difficulty. If, therefore, we have to deal with inflamed parts, we have it in our power to remove the chief conditions of the abnormal production of heat.

But after a time we are even able to reduce the morbid increase of temperature of the entire mass of blood. This fluid, passing in a continued flow from the heated through the cooled parts, has to yield to them a considerable quantity of its warmth. It is true that the observations of Traube, Bergmann, Hoppe, Liebermeister, and others, have shown that abstraction of warmth from the body by exterior causes is soon followed by a proportionate increase of the blood-temperature, and therefore gives rise to an increased production of heat in the organism. It has been justly maintained that a regulating apparatus for the production of heat must somewhere exist, though its exact seat has not yet been made out. If this apparatus acted uniformly under all circumstances, it would, of course, be useless to try to reduce the temperature of the blood by outward cooling. But this regulating apparatus acts only within certain limits.

If the abstraction of heat be continued for a longer period, and if it be applied to a sufficiently large surface of the body, the loss of heat will soon surpass its reproduction, and a decrease of the temperature of the blood will follow, which may be determined by the thermometer.[1]

Thus we have, in the local abstraction of heat, a remedy against fever, and it follows from all that has been stated that there is no other known remedy with which we are able to attack the inflam-

[1] Compare Traube, 'Deutsche Klinik,' 1852, p. 176; 'Charité Annalen,' vols. 1 and 2; Virchow's 'Archiv,' 1858, vol. xv, p. 70. Very interesting experiments on the effect of continued abstraction of heat on the temperature of the body are reported in Mr. James Currie's valuable paper, 'Medical Reports on the Effects of Water, Cold and Warm, as a Remedy in Fever and other Diseases,' Liverpool, 1798. The first volume of this work was translated into German by Dr. Michaelis in the year 1801. The second volume was translated in 1807 by my honoured colleague, Statsrath Professor Hegewish, who supplied his translation with interesting remarks. In perusing this work I was often reminded "that there is nothing new under the sun."

matory process so effectually, from so many sides, as with the local abstraction of heat uninterruptedly extended over a sufficiently long period. This object, however, may be practically effected in many different ways. It is therefore necessary, first, to examine the several modes by which local abstraction of heat may be obtained, and to estimate them according to their respective values.

The following are the chief modes of applying cold which come under our notice in medical practice: compresses, immersions, affusions, and bladders or bags of ice. It is by means of the latter only that we are enabled to apply cold exclusively. In the former modes the effects of moisture of the water are always superadded. We have, therefore, to distinguish between *wet* and *dry* cold.

The most familiar of all methods, but, at the same time, the one least to the purpose and the most uncertain, is the application of cold compresses. One *can* produce by them a continued abstraction of heat, but very often the opposite is effected. If they are not very often changed, the inner surface of the wet compresses will very soon acquire the temperature of the inflamed part, and in this way the ordinary escape of heat will be prevented, and the inflammatory increase of temperature augmented, instead of lessened. With the application of every fresh compress temporary abstraction of heat takes place, but the constant change from heat to cold will give rise to irritation, which, in some forms of disease, may act beneficially, but very often augments the inflammation instead of reducing it.

If very cold compresses be used, such, for instance, as are made cold by ice, and if these be very often exchanged, the temperature of the part may be continuously diminished; but even then the constant touching and disturbing of the inflamed part by unpractised hands would very often cause a hurtful irritation, which would outbalance the good effects of the abstraction of heat. Moreover, in applying these cold compresses, the clothes and bed-linen of the patient often get damp or wetted through, and it is in this way that those bad effects are produced which the adversaries of cold generally enumerate, and which originate simply in giving cold unintentionally to healthy portions of the skin. Besides, a mistake is here not unfrequently made which may be followed by the worst consequences—I allude to the application of cold compresses on the dry bandages with which wounded, broken, or inflamed parts have previously been dressed. I have seen several cases in which

the hand or the arm has become gangrenous, in consequence of having been wrapped up, *lege artis*, in dry bandages, and then treated with cold compresses. This kind of misfortune, indeed, can only happen to medical men who are extremely careless, or who have never heard that a dry bandage contracts greatly as soon as it is wetted; but on referring to the literature of this subject, we find that such incidents have been explained by the adversaries of our remedy as the bad effects of cold, whilst they were simply the consequence of wet, or rather of carelessness or ignorance.

For all the above-mentioned reasons, cold compresses ought to be dispensed with, at least in severe cases; or if necessity compels one to use them, their application ought to be carefully superintended. But truly the time has fully come when the expressions "cold" and "cold compresses," ought not to stand any more for one and the same thing.

Very much more effective, and in many cases well answering the purpose, are cold local baths (immersions) and affusions with cold water (irrigations).

If we place a part of the body for a considerable time in cold water, or if we allow a slender stream to play continuously upon it, we then very energetically abstract heat from the part, fresh particles of cold water being brought in quick succession into contact with its surface. In the case of immersion this effect is produced in the following way:—the particles of water in contact with the part become warmed, and thus give rise to currents which carry them away, replacing them, at the same time, by other colder particles. In this procedure the whole surface of the immersed part is constantly washed by the cooling medium. The effect is therefore comparatively powerful and extended, and hence, as a rule, for fear of bad consequences, we should be careful to avoid, in this mode of application, the employment of too low a temperature.

I saw, not long ago, a boy who had lost all the fingers of his right hand, in consequence of his surgeon having applied continuous baths of cold water, of about 45° F., to a not very considerable contusion of the hand. The patient soon got violent pains in his fingers and in his arm. The surgeon thought that they were produced by the inflammation which had meanwhile set in, and this notion only induced him the more to persevere in the cold-water application. The pains lasted for three days, and ceased only with the death of

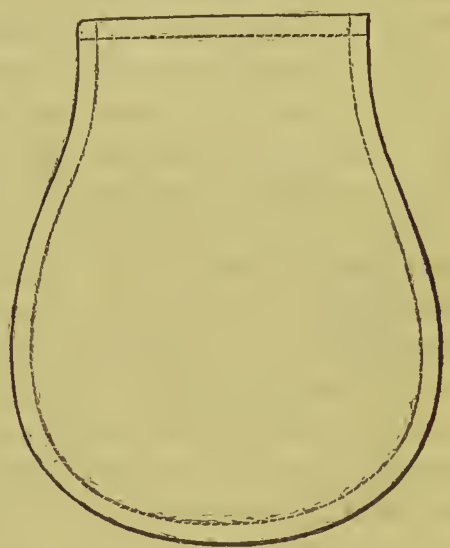
the fingers from the effects of cold, and their separation, one after the other, by gangrene.

To appreciate the difference between the total immersion of a part and a more local application of cold, we need only with one hand take hold of a snowball or lump of ice and dip the other hand into cold water of about 41° F. (4° R.) We shall find that we are able to go on with the former experiment for a considerable time without inconvenience, whilst intense pain, running up the arm in the course of the nerves, will soon compel the withdrawal of the immersed hand out of the water. Tepid baths, from 77° to 94° F. (20 to 27° R.) act, therefore, in our opinion, in an antiphlogistic way, for the difference between the temperature of the body and that of the water is quite sufficient to effect a considerable abstraction of heat from the body. (Compare Langenbeck, Deutsche, Klinik,' 1855, p. 410.) However excellent in their effects continued baths and affusions are in the treatment of inflammatory processes, there are still some disadvantages connected with them which prevent them from being universally applied. In addition to abstraction of heat, we have here also to take into consideration the effects of moisture, which, certainly, are very often not desirable. Swelling up of the epidermis and imbibition by the edges of the wound, in consequence of their constant exposure to wet, are circumstances which may become injurious to the healing process. Further, the effects may extend too far into the neighbouring healthy parts, and there, by giving cold, produce rheumatic pains, which soon become intolerable to the patient. Then, lastly, these two methods are not equally available for all parts. In the absence of more elaborate contrivances, they can only be applied to the hand and arm as far as the upper third of the humerus, and to the foot and leg not higher up than the knee-joint. Besides, in both methods, in affusion and in immersion, the parts concerned have to be placed in a somewhat sloping posture, and hence one loses the advantage of the high position, which I consider of great importance in the treatment of injuries of the extremities. By using india-rubber rings this disadvantage may be in some cases avoided, but I doubt whether the still greater disadvantage of the circular compression which would thus be produced could be avoided with equal success.

We attain our object of abstracting heat continuously from an inflamed part in a far more secure way, and without injurious secondary effects, by applying dry cold, by means of waterproof

receptacles filled with ice, snow, or some freezing mixture. Preferable, for practical purposes, to all other contrivanees, are ice-bags of vulcanized india rubber, and especially those which go under the name of American sponge-bags. Their form, as is shown in fig. 1, is very nearly that of an old-fashioned work-bag, and they have a sufficiently wide opening to admit of their being filled with large pieces of ice. They are much preferable to animal bladders, which are never quite waterproof, and which, after a time, become offensive, because the part above the ligature, which is not exposed to the eonstant influence of the ice, soon putrefies.

Fig. 1.



It is true that moisture from the atmosphere is precipitated on the onter surfaee of the india-rubber bag, so that it always appears wet, but a bit of lint is sufficient to protect the part from damp. The chief advantage which is derived from these india rubber bags is that heat is badly conducted through their walls. A too great abstraction of heat is therefore not to be feared, even in cases where the practitioner is not able to superintend the application of the remedy. The walls of an animal bladder conduct heat much better than an equally thick membrane of india rubber, and we must, in consequence, watch carefully over the application of ice-bladders. This I had the opportunity of remarking in a case which came under my care not long ago. A countryman had got, in consequence of a contusion, acute inflammation of the knee-joint. His surgeon, a very able and careful man, applied two pigs' bladders filled with ice, one on each side of the joint, giving instructions that they should be kept filled with ice as long as the patient felt benefit from them. The distance of the man's house prevented the surgeon from looking in upon him frequently. He did not, in fact, visit his patient again until, after the lapse of a considerable time, he was called in to see, to his great alarm, two large, discoloured, frostbitten patches, of the size of a child's hand, oeeupying the places where the ice-bladders had

touched the skin. The man felt great and rapid relief from the application of the ice. The violent pains in the joint disappeared, but as soon as the abstraction of heat became too vigorous his joint began to grow painful again. The patient attributed these new pains, not to the ice, but to a fresh onset of inflammation. He therefore persisted in the application of the ice till the parts were killed. It was after this that he was taken into the hospital and put under my care. The discoloured parts soon separated, and left granulating surfaces behind, after the cicatrization of which every sign of the inflammation of the joint had disappeared.

In using ice in india-rubber bags such an incident, I think, need not be feared. The abstraction of heat by means of this appliance never becomes so powerful as to bring the circulation entirely to a stop, or fully to kill the tissues. If such a bag of ice has been for some time in contact with a part of the body, the part feels as cold as ice, but has by no means lost its sensibility. I shall hereafter show that an inflamed part may be kept, without interruption, for months together, at that temperature without the least damage being caused. The chief objection which may be raised against a very extended use of these india-rubber bags is that they are rather expensive. A good, middle-sized bag is not to be had under eighteen-pence, and the larger ones cost three shillings and more. But, on the other hand, they are very durable, if they are made of a good material and if carefully used. Those made in America are by far the best; they are of a light-brownish colour, their surface is uniformly smooth and even, and their walls are comparatively thick and heavy. They bear the stamp "Godyear's Patent," and are to be got at Mr. Schliemann's, in Hamburg, who has five different sizes in store. I have used some of them continuously for nine months before they got damaged. Those made in Europe—German, English, and French—are of a much inferior quality. They are of a light-gray colour, and very often present on their surface rough spots, which are small bits of sulphur that, in the process of valcanizing, have not combined with the india rubber. These become detached during use, and leave little depressions behind, or sometimes even perforations. It is of great importance to deal carefully with them; the smallest damage is irretrievable, and soon ends in a tear. If the bag ceases to be waterproof it, of course, entirely loses its usefulness. To avoid tearing, the mouth of the bag must not be too narrow, as is, for instance, the case with some of the flask-

shaped kinds made in Germany and France, for in this case the nurse who has to fill it with ice will probably injure it in endeavouring to force a too large piece through the narrow opening.

Another important point is the mode of closing.

Formerly we used to draw the orifice of the bag together with a piece of tape, in the way that leather purses are closed. But to shut up the bag well by these means it is necessary to draw the tape very tight, and as this has to be repeated every time the bag is filled, it soon gets damaged. But by using a large cork or, better, a wooden roller, the bag may be hermetically closed with much less injury to it. The opposite sides of the orifice should be placed smoothly in apposition, and then tied round the roller (fig. 2) with a bit of tape sufficiently broad (about half an inch) to prevent it from cutting into the substance of the bag. The mode of fastening is represented in fig. 3. The roller has in its centre a ring, through which a bit of string may be passed, with the object of suspending the bag over the part of the body to which the ice is to be applied in cases where it is desirable to prevent pressure. The bag, thus closed, may be hung up with its orifice downwards for days together, without allowing of the escape of a single drop of water. If the inflamed part can bear, without inconvenience, a moderate amount of pressure, and if it is not necessary that the patient should keep his bed, the ice-bag may be wrapped up in a cloth and tied round the injured part, and thus made to fit nicely. It is in all cases advisable to place a bit

Fig. 2.

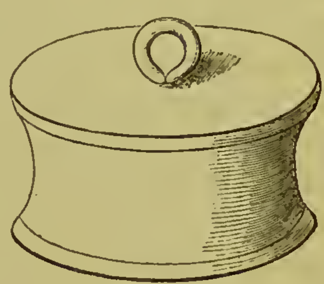
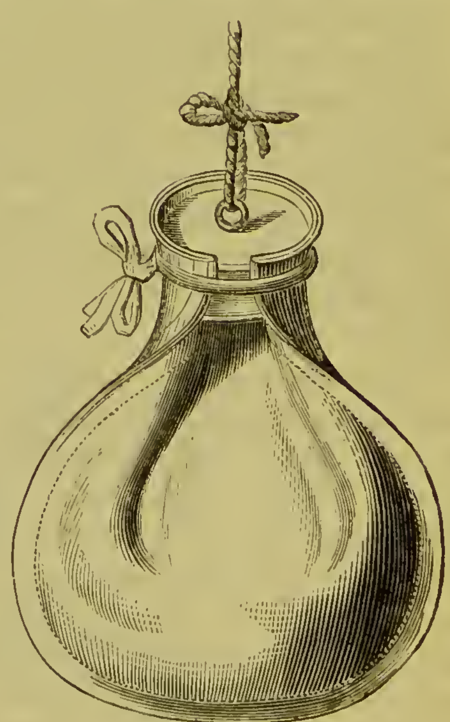


Fig 3.



of lint between the ice-bag and the surface of the body, for to many patients it is inconvenient to have the india rubber in constant contact with the skin. Besides, by varying the number of layers of lint, between the ice and the part, we may adapt the degree of cold to the requirements of the case.

It may be fairly objected that this mode of using ice is still too expensive for many patients. In less severe cases, especially, we cannot well expect a poor person to provide himself with india-rubber bags and to spend his money on pails of ice, of which a large quantity melts away without being used. Moreover, ice is not to be had at all places and at all times, so that it may be said that if, under such circumstances, we wish to use cold, we must, after all, use it in the form of cold wet compresses.

Now, in Holstein, at my predecessor, Professor Stromeyer's, request, ice has been added to the pharmacopœia, so that every apothecary is bound to have it always in store; besides, the use of ice, and with it the number of ice-sellers, increases from year to year; but, nevertheless, there are times in which the provision of ice is very scanty. This consideration, together with that of the means of the poorer classes of patients, has induced me to think of a method of applying dry cold in another and less expensive form.

If an india-rubber bag be filled with cold pump-water, its power of abstracting heat is but very small, because india rubber is a bad conductor of heat. A pig's bladder, or a bit of gut, often refilled with fresh, cold water, will be found much more effective.

To make such animal membranes waterproof, we must rub them well on both sides with lard or oil. But even then they become permeable after a time, and often get to smell so offensively that they have to be exchanged for new ones.

Glass, and especially metals, are, as every one knows, very good conductors of heat. By means of them we are able to obtain considerable effects with ordinary fresh pump-water, and for some years past, I have been led to employ this mode of applying cold more and more extensively. I am in the habit of ordering for out-patients, instead of cold compresses, cold bottles, and have seen excellent results therefrom. Even the poorest patient can easily provide himself with an ordinary medicine-bottle, or with a wine-bottle. If such a one be filled with cold water and well corked, and then applied to the inflamed part, the patient at once feels the comfort of its cooling influence, which lasts till the water has been warmed to a

considerable degree, when, of course, it must be replaced by a fresh quantity. In the winter I make the patients fill their bottles with snow or with ice, and tell them to wrap them up in a handkerchief or two if the cold be too powerful.

On account of their convexity, ordinary bottles do not admit of being equally well applied to all parts of the body, offering only a few points for contact. For the palm of the hand, the armpit, the groin, and the neck, an ordinary mixture-bottle does very well; the forearm and the calf may rest comfortably and cool on two wine-bottles filled with cold water; for other parts, as, for instance, the breast and the abdomen, we may use flat bottles; but for some parts of the body, such as the back, bottles are in no way applicable. In these latter cases, and, in fact, everywhere where my object is to bring larger surfaces of the body in contact with the cold-water-reservoir, I use receptacles of thin iron plate, one side of which has been made exactly to fit the surface of the part for which it is intended. The cooling effects of such iron-plate reservoirs, when filled with cold water, are excellent, and I strongly recommend this mode of application, especially in cases of inflammation of the vertebral column, where in no other way the continued abstraction of heat can be so effectually obtained.

In all cases of vertebral inflammation in which local or excentric pains are present, and without reference to the existence of Pott's curvature, I at once have a mould taken from the affected part of the back. This is best effected with a sheet of gutta percha. A box of thin iron plate is then made, the upper surface of which is exactly formed after the mould, so as accurately to fit the inflamed part. At the side of the box there is a small opening, which may be closed by a cork, and through which, by means of a funnel, the box may be filled with cold water, or emptied, when required, by help of an elastic siphon, and all this without being obliged to move it in the least.

Fig. 4.

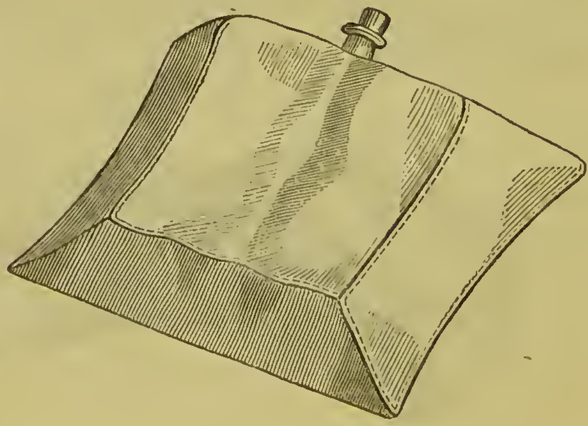


Fig. 4 represents a box which was used

in the case of a young girl who suffered from inflammation of the middle dorsal vertebræ. The apparatus was applied to the parts for fully six weeks, with the best results.

Fig. 5.

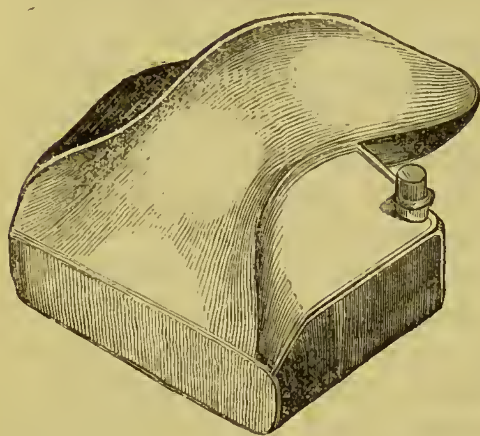


Fig. 5 is a similar contrivance for the neck. A young boy, fifteen years of age, who suffered from rheumatic inflammation of the vertebræ of the neck, followed by very extensive inflammatory exudation, lay on this box for six weeks, till the pains had quite disappeared, and till the exudation had been almost entirely reabsorbed.

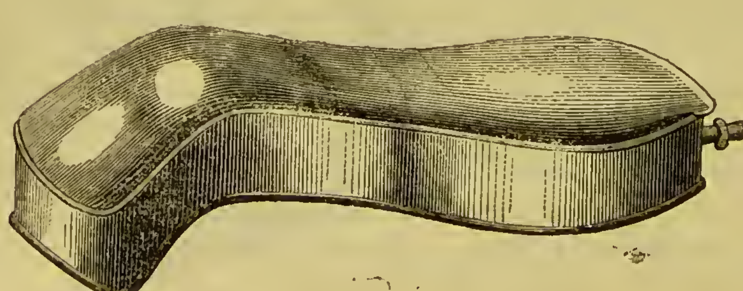
These cold cushions, as they may be called, are introduced between the bedclothes, so as to allow the back to rest comfortably upon them. I can assert that the patients feel quite at ease on this kind of bed, and that under this treatment the pains will be found to subside much sooner than under any other.

I have not yet come to any decision as to the importance of letting patients who are suffering from advanced inflammation of the vertebræ lie on their belly. When I have required them to assume that position I have, of course, used ice-bags.

I have also applied similar contrivances, to those represented in figs. 4 and 5, to the extremities. I have then had the upper surface of the water-box, on which the part was to rest, made of a wooden splint.

Fig. 6 represents an apparatus of this kind made to fit the arm of an adult.

Fig. 6.



The parts adjacent to the diseased texture, which are not to be exposed to the cold, may be covered with a thin layer of cotton-wool. These cold-water-boxes are, I confess, in the first instance rather expensive, but when once procured, as for hospital use, they are of great advantage, much ice being saved by their employment. In bad cases, of course, they do not obviate the necessity for the application of ice-bags, &c.

We now turn our attention to the indications which are to guide us in the use of the constant abstraction of heat, and it may be stated in general terms that the more acute and severe the inflammation is, and the more important the inflamed part for the organism, the more urgently indicated is the application of this treatment. The direct effect of the treatment is, of course, more powerful in proportion as the inflamed part is nearer to the surface of the body, but deeper-seated parts are by no means inaccessible to cold. We observe that in inflammation of internal organs, such as the lungs, heart, abdominal cavity, &c., the treatment by cold is becoming more general.

To determine how long a time we ought to proceed with the application of cold in a particular case is a more difficult question. As long as there is any abnormal increase of temperature, the abstraction of heat acts, as a rule, beneficially. But we shall probably come to a much more accurate estimate, and shall be able to determine with the thermometer how long the remedy is to be continued, in the same way as Traube, Wunderlich, and others, take measurements of temperature for their guidance in the treatment of fever cases. I have occupied myself some time now with measurements of the kind, but have not come to any conclusion yet as regards this point.

But it is certain that the use of cold is not indicated in the first stages exclusively of acute inflammation. The setting in of suppuration is by no means a reason for discontinuing the abstraction of heat, nor is even the occurrence of gangrene, if it be caused by the severity of the inflammation. These processes, on the contrary, run generally a much more favorable course under cooling treatment than under the so much bepraised use of cataplasms.[1] Generally, we may be guided by the feelings of the patient. In severe injuries and violent inflammations they feel relief from cold sometimes for

[1] Compare Prof. Stromeyer's 'Maximen der Kriegsheilkunst,' 2nd edition, p. 310.

months together, and I have several times observed that in such cases the inflammatory process has revived when I have discontinued the cold, and that the patients have implored me to let them have the ice again. If the abstraction of heat gets disagreeable to the patient, if he begins to complain of chilliness of the whole body and dragging pains in the inflamed part, we must, in the first place, make sure that the cold is not being injudiciously applied.

It happens often that the degree of cold is too high for the case of inflammation which we have to treat, and then it is sufficient to regulate it by placing a pad of lint between the ice-bag and the part, or to diminish the surface of contact. Generally, the uncomfortable feeling subsides at once, but where this is not the case we must discontinue the cold altogether; this must be done gradually, to avoid a too powerful reaction in the part.

There are, indeed, certain individuals who, under any circumstances, even if every possible care be taken, cannot bear the application of cold, but these are very exceptional cases. On what causes this curious idiosyncrasy really depends, I have not yet been able to ascertain; where we meet with it we must not enforce the use of our remedy, which might prove dangerous, but must try our luck with other antiphlogistic methods.

It is by no means merely in acute inflammatory processes that cold is such an efficient remedy. It has proved serviceable to me also in so many cases of chronic inflammation that I always feel it my duty in such cases to try, in the first instance, what may be done by it. Although it certainly retards the local changes of the part, it by no means prevents the absorption of extravasated blood and of inflammatory products, but, on the contrary, hastens it in the proper cases. In chronic inflammation of the joints I have not unfrequently applied bags of ice for months together, and have seen under such treatment the swelling and other symptoms decrease, slowly, it is true, but continuously.

If we now specially consider those morbid conditions for the relief of which we apply our remedy, we shall find that the effect of the cooling treatment becomes, of course, most visible in those cases in which the inflammation tends to develop itself in area and in intensity.

Wounds of all kinds heal often rapidly and without suppuration under the influence of a moderate and continued abstraction of heat. But let it be understood that I am by no means of opinion

that all wounds must absolutely be treated by cold. This would be denying the healing power of nature. I have often enough seen simple and clean-cut wounds healing by first intention, without anything else having been applied but sutures and a compress or a bandage. Some wounds, as, for instance, those with loss of skin after operations, must be left to heal with suppuration. In such cases, by continuously applying cold, we should probably retard the cure. I therefore use it only during the first day, to relieve the pain of the wound, and never proceed with it except there is reason to fear that the inflammation may spread to important organs.

I am also well aware that even very severe wounds heal sometimes in a remarkably short period without the application of cold, when the natural healing process is not interfered with. This is the chief reason of the efficiency, in cases of compound fracture, of firm bandages (starch, plaster of Paris, &c.), to which their advocates attribute such great antiphlogistic powers. They protect the injured part from many bad influences which are apt to disturb the healing process, especially from motion, exposure to air, and improper treatment. I shall not complain if, in this sense, antiphlogistic power is attributed to them.

Although firm bandages fulfil, in the most simple and certain way, the chief condition, namely, absolute rest and immobility of the injured part, which is requisite to the favorable progress of the healing process, they have, on the other hand, the disadvantage of concealing the part from our eyes, and of preventing us from interfering in time with any inflammation which may arise. For this reason they will probably never supersede the treatment by cold in the worst forms of injury, namely, gunshot wounds.

In these cases it is generally inflammation of important parts, especially of bones and joints, which tends to render the issue of the case an unfortunate one, and which, at any price, must be suppressed at the very beginning. During the Danish war the surgeons of the Schleswig-Holstein army had ample opportunity of convincing themselves of the excellent effects of the cooling treatment in cases of gunshot wounds. The results have been published, partly by myself, in my paper on 'Resectionen nach Schusswunden,' but especially in Stromeyer's 'Maximen der Kriegsheilkunst,' and to these writings I now refer. At that time I was but a novice in surgery, and although I was fully convinced of the excellency of this method of treatment, I had but little experience of the results of other

methods to compare it with. Soon after the close of the war I had the best opportunity of making such comparisons on the very largest scale.

In the autumn of 1851 I went to Paris, and visited there diligently the surgical wards and hospitals in all parts of that large city. Suddenly they were filled with wounded of all descriptions, in consequence of the massacres and street-riots which took place on the 4th and 5th of December. I felt great interest in the progress and treatment of these various wounds, and gave my whole attention to them, visiting especially those wards into which the wounded had been taken.

Those who are acquainted with Paris know that it is almost impossible to visit more than one hospital in the day, in consequence of the great distances, and because the visits are made in almost all hospitals at one and the same hour in the morning. I had, therefore, no other course to take than to visit in a certain succession those wards which contained the greatest number of wounded, and, with the help of notes, to keep a record of the severest cases.

I had a right to trust my judgment in the matter, as during three campaigns I had watched, and had myself treated, a great number of gunshot wounds. When the lectures of the Paris surgeons, and their discussions in the Academy of Medicine, on the gunshot wounds which had occurred in February and June, 1848, were arranged and translated into German in 1849 by Dr. Wierer, from the reports of the 'Gazette des Hôpitaux,' I read them with the greatest interest, and was, as well as my colleagues, very much astonished to find what diametrically opposite views, partly sustained by statistical statements, were held by the most celebrated French surgeons on the most important points. Now I had an opportunity of watching with my own eyes the results of such various views and methods of treatment, and what I saw by no means diminished my astonishment.

Some, perhaps, who have read the book just referred to may have formed the opinion that success in surgery is due more to good luck than to the method of treatment; to me it has become most evident, after the observations I myself made on the spot, that success in surgery is essentially due to the principles which the surgeon follows in the treatment of his patients.

First of all, I found a great difference in the way in which army and civil surgeons treated their cases.

I shall enter into it only as much as it concerns us here.

In Gros-Caillou, the large military hospital near the Champs-de-Mars, there were a number of severely wounded soldiers. Cold was used in these cases with great perseverance. I found the limbs, of which the bones had been smashed, well put on splints, and enveloped with Scultetus's bandages. In the severest injuries, such as gunshot fractures of the leg, there were placed on the bandages a number of small pieces of ice, which were replaced by fresh ones as soon as they had melted. The water dripped off over a piece of oil-cloth into a pail. On those amputation-stumps, also, which were bandaged there were always bits of ice melting. The ice was not clean, and on the bandage there was soon formed a thick layer of black dirt; but the smashed limbs and the amputation-stumps were, for all that, in a very good state. I was present when, on the eighth day, the bandages were removed from all these wounds, and, if necessary, replaced by new ones.

Almost all the patients looked as if there was nothing the matter with them, and although, according to my opinion, the manipulation in bandaging was by no means faultless (the pus, for instance, was squeezed out of the wounds, the stumps were lightly compressed with sticking-plaster, &c.), all the severe wounds presented an excellent appearance. Nowhere were to be found any of the alarming swellings and infiltrations in the neighbourhood of the gunshot fractures which are so difficult to get rid of when they have once appeared; nowhere was the suppuration profuse and offensive, but, as is usually the case in the treatment by ice, it was scanty and somewhat serous. This treatment with ice was continued uninterruptedly during four weeks. Then cold affusions were used, which, in the less serious injuries, had been applied from the beginning. In ten days more I found most of the dangerously wounded already with dry bandages. The wounds were nicely granulating, and suppuration was taking place only from the surface; the gunshot fractures were consolidated, and the amputation-stumps nearly healed up. The result was most strikingly favorable in a case of perforating wound of the knee-joint. A spent ball had struck the centre of the patella, and broken it into five fragments, which were visible through the circular opening in the skin. On the seventh day, ice having been continuously applied, the joint was scarcely at all swollen, and the general health of the patient was excellent. At the end of four weeks, although I found the joint much swollen, and the wound, by slight pressure, yielding purulent synovia, it was granulating well

and was already half closed, the surrounding skin was neither red nor tender, nor was there either above or below a trace of inflammatory infiltration; the patient had scarcely any fever, and his general health and his appetite were very good. From this time cold affusions were made use of. By the sixth week the wound was nearly healed, the suppuration was very slight, and the patient's aspect was that of a healthy man. I heard nothing further of the man's progress, but I have no doubt that the wound very soon entirely closed. Now, it is true that of gunshot injuries of the knee-joint those of the patella run the most favorable course, and in my paper on 'Resectionen,' p. 130, I have myself communicated two cases in point, but they take, generally, a very long time to heal, and the patient's sufferings are frightful. What struck me most in the above-related case was, that during the continued treatment by ice the general health of the patient was so little affected. Among the wounded who were in this hospital on the 9th of December, that is, five days after the riot, there were two who at once caught my eye in consequence of their unhealthy aspect. I heard that they had first been taken into the Hôtel-Dieu, and had there, during four days, been treated with warm cataplasms. One of them had a shot through the soft parts of the leg, without the bones being fractured. Perhaps the tibia had been touched by the ball. A severe phlegmonous inflammation spread over the whole leg up to the knee, and large incisions had to be made on the 12th of December, to let the pus out. I saw the patient again on the 18th of December, when the wound was secreting scanty, offensive matter. He showed all the symptoms of pyæmia, of which disease he died a few days afterwards. The second patient had a wound through the middle of the hand. Several bones of the carpus had been smashed to pieces, the hand was enormously swollen, the wound secreted bad pus, the patient had intense fever, and his appearance was such as to induce me to anticipate death from pyæmia. At once continuous cold affusions were applied, in consequence of which his condition improved rapidly. By the 12th of December the swelling was considerably decreased, and the wound looked much better. On the 6th of January the swelling had entirely disappeared, the wounds were granulating favorably, and the general health was excellent. On the 17th of January the wounds were almost entirely closed.

In the other large military hospital, the Val-de-Grâce, cold was also applied to the severe injuries, but much less energetically and

perseveringly than in the Gros-Caillou. The smashed legs rested on splints placed on oil-cloth, and were surrounded and covered by large, wet sponges, which were often renewed. In other cases affusion was made use of by means of small glass siphons. Ice was not employed at all. The other parts of the surgical treatment of the wounds, such as examining and bandaging, was done, on the whole, more judiciously and carefully than in the Gros-Caillou. In some cases the patients were extensively bled. In a case of a perforating wound of the chest, for instance, besides a deal of cupping, the man was bled freely ten times. In the fifth week I found him quite well, and his wound completely healed. Many dangerous cases were cured also in this hospital, although much more slowly than in the Gros-Caillou, and I had occasion to observe more frequently abscesses, infiltrations, and other ill effects of gunshot fractures, so that the bistoury had to be used oftener than at the other place.

A very different appearance from that of the wounded in the military infirmaries was presented by those in the civil hospitals. In one of the wards of the Charité there were about twenty wounded (some only slightly injured), who were all treated from the first by warm cataplasms. I saw them on the fifth day, and found all of them looking very ill, their wounds secreting bad pus, and presenting œdematous or purulent infiltration in the neighbourhood. During the following few days several of the patients died in consequence of pyæmia, and a splendid lecture, given by the clinical director of the ward, on the excellency of warm cataplasms in the treatment of recent injuries, could not shake my opinion that these very cataplasms were partly to be blamed for the bad state of the wounds; although I must confess that a part of the blame was due certainly to the assistants, to whose care these cases were entirely intrusted, and who before the going round of the surgeon busied themselves with syringe and sponge and with squeezing out the contents of the sinuses, thus, according to their fashion, cleansing the wounds.

In M. Roux's wards in the Hôtel-Dieu the wounds presented a much better appearance than those in the Charité, although he also is a decided adversary of cold, and in many cases applies from the first warm poultices; but his other treatment was much more careful. He with his own hands put the bandages on in a most skilful way, and united the edges of the wounds with the greatest care. Nevertheless, the state of his patients and the course their wounds took could by no means compete with those in the Gros-

Caillou. Some of them died of pyæmia who, I am certain, would have been saved in the Gros-Caillou. In the other civil hospitals I observed the same results obtained by the same kind of treatment. In some few cases, where the bandages had been put on with great care, gunshot fractures were, indeed, cured without the help of ice, but the whole process was infinitely more unfavorable and tedious than in the military hospitals.

These comparisons tended necessarily to confirm my opinion of the value of cold, and I returned home with the resolution to test this remedy in all ways—to decide, in fact, what results could really be obtained by it. I have since had ample opportunity of carrying out my resolution, and I think that, on the whole, I have reason to be well pleased with the results. I have a great number of serious cases to adduce which I have succeeded in curing by making use of the antiphlogistic power of cold. I believe, with the help of ice, that I have saved a considerable number of limbs which, without it, would have had to fall before the knife. I have, therefore, no greater wish than to see this remedy more generally introduced into surgical practice.

I think it necessary to prove my assertions by recording the observations I have made in my own practice; I will therefore give the history of a few cases of compound fracture, which will tend to confirm, by results obtained in civil practice, the experience acquired during military service.

Case I.—*Compound fracture; application of ice during eight weeks.*—H. R—, a strong, working-man, æt. 28, got a compound fracture of his tibia and fibula, with considerable contusion and laceration of the surrounding soft parts. This was caused by a heavy sack of grain falling from thirty feet high, out of the trapdoor of a corn-loft, upon his left leg. The patient was at once taken into Friedrich's Hospital. The tibia and fibula were found broken in their middle, and the lower, sharp fragment of the tibia protruded through a large, lacerated wound of the skin. We succeeded soon, by extension, in bringing the bones back into their normal position. The leg was then enveloped with Scultetus's bandages, and placed on a Heister's splint; two large bags of ice were put over the region of the wound. Immediately after the accident the pulse was small and feeble, but became towards evening so full and strong that the patient was ordered to be bled to twelve

ounces. During the next day or two there was some swelling and redness around the wound, but the inflammation was soon subdued by the constant use of ice. The man had no pain, slept quietly at night, and felt well. The wound was now dressed with oil and lint, and the two ice-bags were kept on the leg day and night. At the lower margin of the wound the naked bones could be seen for a time bathed in pus, until, in fact, they were covered from all sides by the advancing granulations. By the ninth week the wound had got considerably smaller, and the fracture was nearly united. The ice-bags were now taken away, as they were becoming inconvenient to the patient. On several previous occasions we attempted to do without the ice, but violent pain soon compelled the patient to implore us to reapply it. By the thirteenth week the wound had cicatrized, without a particle of bone having been expelled. During the thirteenth week the patient left his bed, and some weeks afterwards the hospital, with an entirely useful and scarcely shortened leg.

CASE 2.—*Compound fracture; application of ice for seven weeks.*—A. S—, a boy, æt. 8, was run over by a railway carriage drawn by a horse, and had his two legs fractured. There was a simple fracture of the left thigh, and a compound fracture of the lower part of the right leg. The tibia and fibula were broken in their middle; in the skin of the calf there was a lacerated wound, nine inches long, through which the contused muscles were protruding. The wound was now closed by a suture, the broken limbs were placed on cushions, and judiciously fixed. An ice-bag was put on each of the fractures. The pain, which was very violent at the beginning, soon disappeared completely, so as to allow the boy to sleep the whole night through. The following day the right leg was considerably swollen; the sutures were therefore taken out, in consequence of which the edges of the wound separated to a distance of three inches. During the following day or two the swelling went down, and although, at the place where the tibia was fractured, a bit of discoloured skin, two inches in diameter, became gangrenous, the treatment by ice was continued with great perseverance. Under this plan of treatment the wound assumed a favorable appearance, healthy granulations soon sprung up, and laudable pus was secreted. The general health of the patient was all the while scarcely affected. The pulse remained quiet, sleep and appetite were always good. After seven weeks the

cold became inconvenient to the patient, and warm compresses were used instead, subsequently a solution of nitrate of silver. At the spot where the sphacelation of the skin had exposed the tibia a small particle of bone was expelled. By the tenth week the fracture was firmly united, but the wound was not completely cicatrized till the fourth month. To the left thigh ice was applied only for five days. The fracture was healed in eight weeks, with scarcely any shortening. The patient left his bed after the wound was cured, and could then walk well without crutches.

Case 3.—*Gunshot fracture of the tibia; application of ice during six weeks.*—C. P—, a powerful man, æt. 27, was out hunting, and whilst climbing over a wall received in his left calf a charge of small shot from a gun fired close behind him. The shot entered his leg just above the malleolus externus, and passed out in front at about the middle of the tibia. The bone was at this spot found to be much comminuted. The opening by which the shot had entered was circular, and of about two inches in diameter; that by which they had escaped was irregular and lacerated. Extensive hæmorrhage had taken place from the wound, and as I was afraid of its occurring again, and dared scarcely entertain any hopes of saving the limb, I proposed to the patient the amputation of the leg. He, however, resolutely opposed its being done. I therefore removed a number of loose fragments of bone and some small shot from the wound, placed the limb on a Heister's splint, and had two ice-bags put on the upper surface of the leg. The patient assisted our endeavours by showing the greatest patience and quietness during the most violent suffering. The ice was borne well for six weeks, during which extensive suppuration took place; the lacerated tissues, and many small bits of bone separated; the wound became filled with healthy granulations, and the fracture healed. At the end of November the wound was closed, with the exception of a few small sinuses, through which eventually some more fragments of bone found their way out. The patient was now discharged, at his own request, as nearly as possible cured, and presented himself again the next year, having walked about ten miles from his home to the hospital.

Case 4.—*Fracture of the lower part of the humerus; application of ice during twenty-two days.*—M. W—, a boy, æt. 16, fell from a

cart upon his left elbow, and fractured the lower part of his humerus. The line of fracture extended from behind and above, downwards and forwards, right into the joint, which was filled with extravasated blood. The under, sharp fragment of bone was felt protruding a good deal above the internal condyle. After the fragments had been adjusted, the arm was put comfortably upon a cushion and well fixed. Ice was then used for twenty-two days, till the patient could not bear the cold any longer. By this time the arm had reassumed its normal shape. A starch bandage was now put on, and when at the end of the fifth week it was removed, the fracture was found to be united, and the joint to have completely retained its mobility.

In all cases of fracture which involve joints I follow the principle taught by Professor Stromeyer—to subdue all inflammation before making use of firm bandages—and I have never had occasion to regret it. But, on the other hand, I have seen cases treated by other practitioners turn out very badly when firm bandages have been applied at too early a stage.

The various forms of inflammation of joints are certainly some of the most important affections which civil surgeons are called upon to treat, because, if not, from the first, rightly and energetically treated, they are very often followed by loss of a limb or even by death.

In surgical hospitals, at least in mine, recent and acute forms of inflammation of joints come very much less frequently under treatment than neglected chronic forms, such as the so-called white swelling of joints and the suppuration and carious destruction of the knee-joint, hip-joint, vertebral column, &c.

Some surgeons take at once for granted that such cases are of a dyscrasial origin, class them under the name of scrofulous disease of joints, and apply their treatment more to the supposed dyscrasy than to the local affection. I do not, of course, deny that some low inflammations of this kind in scrofulous patients are to be looked upon as symptoms of this dyscrasy, but it is also certain that surgeons very often go too far in classifying cases under the head of scrofula which, in fact, are nothing but neglected and lingering inflammations of a simple kind, which have originally been caused by external influences. When such patients are sent to my wards with the diagnosis of scrofulous affection of the joint I am very often able, on careful examination, to make out that the disease has taken some such course as the following. At the beginning the joint was hurt, perhaps, not very severely, by a blow, a fall, a sprain, or exposure to

cold. When it became painful, and its motions interfered with, a domestic remedy, "something to rub in," was probably employed. If the patient sought the advice of a medical man he was also probably ordered a "something to rub in," or, at the utmost, some leeches or the cupping-glasses were applied, and all the while during the treatment he was allowed to move his limb. If, as might have happened, the medical man required the patient to keep his limb for a considerable time at absolute rest, or, what would be still more trying, not to leave his bed, the patient probably soon left him for another doctor, who, in his turn, prescribed "something to rub in;" or he went, perhaps, to a bone-setter, who stretched the diseased joint violently; or consulted even a homœopathist, who gave him nothing but globules. In this way, after all forms of quackery have been tried, the patient is at last compelled to come to the hospital for relief from his obstinate and uncontrollable affection.

If, in consequence of such mismanagement, patients have suffered continuous pain, have passed many sleepless nights, and, moreover, have lived in damp, ill-ventilated rooms, we need not wonder at their becoming very weak and presenting distinct symptoms of scrofula.

In the treatment of no other surgical disease, perhaps, is the principle "principiis obsta" of such importance as in that of joint-affections. In all recent cases, whether they have arisen in consequence of contusion or laceration, or have been caused by rheumatism, whether the joint be filled with extravasated blood, serum, or pus, it will be always found that the treatment by ice, if judiciously applied, will give certain and rapid relief. It is, of course, necessary that, during the whole process of cure, the limb be kept in absolute rest, and therefore it should be placed and fixed on an apparatus which will answer this purpose. The patient also, as a rule, should remain in bed, but we must be careful that he gets fresh air to breathe and wholesome and nourishing food to eat. In cases of slight disease I simply apply cold locally, in more severe cases I promote the effect of this remedy by local bleeding. I only order general bleeding in the most severe cases of traumatic inflammation and in strong subjects. If the inflammation does not disperse, but becomes chronic, I give mercury internally, and where the bones are especially affected make use of iodide of potassium. So soon as, under this kind of treatment, the inflammatory symptoms have disappeared, the abstraction of heat generally begins to get disagreeable to the patient. I then discontinue the ice, and apply for some little time

mild local irritants, as, for instance, a concentrated solution of nitrate of silver, or something of the kind. After this I put on a firm bandage of starch or plaster of Paris, and this I make the patient wear for about six or eight weeks, till the joint has recovered its normal state. In those chronic cases where there is neither pain nor an abnormal degree of temperature, but only an indolent swelling (tumor albus), continuous abstraction of heat is generally of no use. It is in these cases, above all, that firm bandages are efficacious, especially the plaster-of-Paris bandage and the so-called Scott's bandage (the joint having been previously covered with mercurial ointment). Further, in addition to general strengthening treatment, sea-bathing is to be recommended, in which case, of course, the bandage must admit of being taken off by the patient during the bath.

Where the process has proceeded so far as to have destroyed the articular cartilages and the ends of the bones, we shall, generally, be unable to effect any favorable result with this kind of treatment, however judiciously and perseveringly we may apply it. The only means we then have to save the life of the patient is amputation of the limb, or resection of the joint. But even in these latter cases I have several times, by continuous abstraction of heat, effected very unexpected results, of which I will relate some examples.

In confirmation of the statements I have made above I select, from many cases, the following.

Case 5.—*Traumatic inflammation of the knee-joint after a perforating wound of the joint; application of ice for four weeks.*—H. S—, a powerful, working man, æt. 30, was taken into the hospital in consequence of a perforating wound of the left knee-joint, which he had inflicted upon himself accidentally in the morning by means of a hatchet. The wound was at the inner side of the patella, and was one inch long. By pressing the patella the synovia could be made to ooze out. The edges of the wound were well united with sutures, the leg was placed carefully on Heister's splint, and two bags of ice were applied to the joint. In the afternoon intense inflammation of the joint set in, with violent pain. In spite of the ice, the skin of the part became hot and red, and the interior of the joint filled with fluid exudation. The pulse was full and hard. Twelve ounces of blood were taken from the patient, and half a grain of morphia was ordered. The intensity of the pain diminished, but not the swelling. During the following few days, ice being

still continuously used, from twelve to twenty leeches at a time (in all 204) were, on several occasions, applied. On the sixteenth day pus, mixed with blood, oozed through the wound. The lower suture was therefore taken away, and now the slightest pressure on the distended cavity of the joint made a quantity of healthy pus flow out of the wound. But pressure was carefully avoided, and the application of ice was energetically continued. If it was left off for a moment the patient felt at once the most violent pain. The suppuration soon began to get less. No more pus oozed out of the wound, the swelling of the joint diminished more and more, and at the end of the fourth week the ice had to be discontinued, as it became disagreeable to the patient. At this time the wound was almost entirely cicatrized, the swelling of the joint had nearly disappeared, and the general health of the patient was excellent. As soon as the wound was skinned over, a starch bandage was put on, and the patient went with it out of the hospital. After four weeks the bandage was removed; the joint then presented a normal appearance, and its power of motion was scarcely impaired.

Case 6.—*Wound of joint; application of ice during eight days.*—H. M—, a working man, æt. 17, wounded himself with a straw-chopping instrument in the back of his right hand. The wound was deep and irregular. The tendon of the extensor of the second finger was divided, and the second metacarpo-phalangeal joint was opened. A piece of bone, covered with cartilage, of the size of a lentil, and belonging to the articular surface of the second metacarpal bone, was cut off and was hanging loosely from the capsule of the joint. This small bit of bone was entirely removed, and the wound well closed by sutures. The hand was carefully fixed on a splint, and an ice-bag was placed on the wound. At the end of eight days the wound had healed by first intention, and the joint had completely retained its power of motion.

Case 7.—*Suppuration of the knee-joint in consequence of a perforating wound; application of ice during seven weeks.*—H. S—, an agricultural labourer, hurt his knee-joint with a hatchet, making a perforating wound at the outer side of the patella. Immediately after the accident the wound was closed by means of sutures and treated by cold compresses, repeated local bleeding, and calomel. Under this treatment the wound skinned over, but violent inflam-

mation of the knee-joint supervened, for which now, five weeks after the occurrence of the accident, the patient came to our hospital. The knee was bent at a right angle, was much swollen, red, and fluctuating, and was extremely tender when touched, or when any attempt was made to stretch the limb. The patient was very feverish, his pulse being 144; he had no appetite and was very emaciated, in consequence of the pain which tortured him day and night.

The limb was at once comfortably and securely placed on Heister's splint, two ice-bags were applied to the joint, and morphia was ordered. From this moment the symptoms of inflammation and fever diminished, and after four days the patient slept all the night through without the help of morphia. After cold had been used continuously for seven weeks, calomel in small doses having been administered during the time, the patient was almost completely cured. There was no pain whatever in pressing and moving the joint, and the swelling was scarcely visible. But adhesions had formed, which prevented the limb from being completely extended. I was afraid to extend the limb forcibly, for fear of reviving the scarcely subdued inflammatory process. The patient was therefore sent out of the hospital for a time with a starch bandage. A year afterwards he was put under the influence of chloroform, and I extended his limb with the best result.

Case 8.—*Inflammation of the sacro-iliac synchondrosis; application of ice during four weeks.*—M—, a woman, æt. 40, had been delivered with great difficulty of a very large child six weeks previous to her admission into the hospital. Intense inflammation developed itself in the region of the right sacro-iliac synchondrosis. The part was distinctly swollen, and extremely tender on pressure from the outside as well as per vaginam. The patient was scarcely able to drag herself along. In standing, the whole weight of the body rested on the left leg alone, the right half of the pelvis was drawn upwards, and the tip of the right foot scarcely touched the floor. Any attempt to put the foot to the ground was followed by the sharpest pain, and even when she was at perfect rest the pain was so great that it kept her awake all night. The patient had much fever and was very weak.

On admission into the hospital she was at once placed comfortably on her left side, and a large ice-bag was applied to the painful part.

In a few days the patient was essentially better, and slept well

without the help of morphia. The ice was used continuously during four weeks, after which time the pain and the swelling had entirely disappeared, and it was only when strongly pressed per vaginam that the affected part felt still somewhat tender. The cold began now to be disagreeable to her. It was discontinued, and concentrated solution of nitrate of silver was used locally for the next four weeks. The woman was then entirely cured, and left the hospital without a trace of lameness.

Case 9.—*Chronic purulent inflammation of the knee-joint; application of ice for twelve weeks.*—Christine R—, a girl, æt. 16, had been suffering six years from inflammation of the left knee-joint. It was said to have arisen spontaneously, and the pain was so severe that she had been compelled to keep her bed during the last few years. She came to the hospital to have her limb amputated. The knee was swollen to the size of a man's head; it was bent at a right angle, and the skin over it was red, hot, and tense. At several points distinct fluctuation could be felt, and perforation was imminent. The pain which followed every touch or movement, and which came on also often by itself, was so intense that the patient scarcely got any sleep. She presented a cachectic appearance, was very feverish, the pulse being 150, and had entirely lost her appetite. I thought it scarcely possible that the limb could be saved, but it appeared advisable to delay amputation till the general health of the girl had somewhat improved. The leg was therefore placed comfortably and securely on Heister's splint, and to each side of the splint an ice-bag was applied. The next night the patient slept more comfortably than she had done for a long time, and during the following few days her state improved rapidly. The fever as well as the pain diminished more and more. After a fortnight the swelling was considerably decreased, fluctuation was scarcely to be felt, and the joint was but little tender on pressure. After the ice-bags had been continuously applied for twelve weeks they were removed, because the patient no longer derived comfort from them.

The joint was then very little swollen, and no longer tender on pressure. The girl had entirely recovered her general health, and looked even blooming.

Concentrated solution of nitrate of silver, in the form of a paint, was used for four weeks longer, under which treatment the remaining swelling disappeared so entirely that it was difficult to detect

that the affected knee was still larger than the healthy one. The power of motion was almost completely restored, and the patient, at the end of the sixteenth week, left the hospital perfectly cured.

As I have already had occasion to mention, the cooling treatment is very efficacious in the different forms of inflammation of the vertebral column, for which diseases we have otherwise so few efficient remedies. It is self-evident, of course, that in these cases also absolute rest is the first condition of cure. The patient must lie in bed, on his back or on his belly, till every trace of inflammation has disappeared. I do not deny that cases have been cured by simply carrying out this part of the treatment, especially when the patient has happened to be in favorable circumstances—has had strengthening diet and plenty of healthy, fresh air. It is still pretty generally the custom to apply to such cases in the neighbourhood of the affected part setons, issues, or the actual cautery. I do not venture to give a positive opinion as to the efficacy of these applications; but it seems to me, at all events, questionable if in scrofulous and weakened subjects it is advisable to establish suppurating surfaces, as it is well known that in such subjects the formation of an abscess is not unfrequently followed by the outbreak of many others in different parts. Some time ago I used myself to treat patients suffering from this kind of disease by the above-named powerful counter-irritants and derivatives, but lately I have entirely given up this mode of treatment. Perhaps the chief way in which these remedies act beneficially is that those parts which have a suppurating and tender surface over them are involuntarily kept somewhat more quiet than they otherwise would be. Be this, however, as it may, of one thing I have thoroughly convinced myself, namely, that the inflammatory affections of the vertebral column are much more rapidly cured under the treatment by cold than under any other. I will relate the following cases as examples.

Case 10.—*Inflammation of the cervical vertebræ; application of ice during six weeks.*—H. T—, a merchant's apprentice, whose cervical glands had been from childhood hard, swollen, and suppurating, got, in consequence of a cold, a very painful inflammation of the cervical vertebræ. This inflammation made rapid progress, in spite of the use of leeches, of iodine ointment and of cod-liver oil; the patient, therefore, came to our hospital. His head was then bent forward to

such a degree that his chin was almost touching the sternum. The cervical portion of the vertebral column was surrounded by firm exudation, so that the much swollen region of the neck felt as hard as wood. The upper half of the cervical spine was bent forwards upon the lower at an obtuse angle, the spinous process of the fourth cervical vertebra being the most protruding part. The swollen portion of the neck was very tender to the touch, especially in the region of the third and fourth vertebræ, but the pain was most intense when the patient made any attempt to move his neck, under which effort the scaleni and the right sterno-mastoid muscle became very tense. Great pain was also felt in walking and when the head was pressed from above upon the vertebral column. There were no paralytic symptoms, but the patient was very much weakened, and presented altogether a most pitiful aspect. Under these circumstances the prognosis seemed to be a very unfavorable one.

An ice-bag was applied to the neck, and every fourth day an artificial leech was put on. After this treatment had been persevered in for six weeks the tenderness had quite disappeared, and although the swelling had but partly subsided, the head could be straightened much more freely than before. The ice now became disagreeable to the patient, and was, in consequence, discontinued. To hasten the absorption of the still existing exudation, an issue was made in the region of the neck, and one grain of calomel daily given to the patient. The progress towards cure under this treatment was slow, but continuous. After the lapse of six months the patient was perfectly cured, and left the hospital with a completely moveable neck.

For a similar case, which came some time afterwards under my care at a season when ice was not to be procured, I had the water-reservoir made which is represented (fig. 5), and used it for six weeks with the same good result as the ice in the previous case.

I have also obtained equally good results from cold in numerous cases of inflammation and suppuration of the dorsal and lumbar vertebræ. The best effects are, of course, obtained in recent cases, where Pott's curvature has not yet formed, and where, with the exception of tenderness on pressure in the region of one or more spinous processes, and of excentric pains, and pain in jumping and striking the head, there is nothing to indicate the existence of inflammation. But even in cases where there is already angular curvature—that is, where at least the body of one vertebra has been destroyed by sup-

puration, and where abscesses have formed—the use of cold will not be unattended by beneficial effects.

Case 11.—*Pott's curvature, formed by suppuration of the eighth and ninth dorsal vertebræ; fistula in the inguinal region from the opening of psoas abscess; application of iron-plate box during six weeks.*—Margaret B—, a servant girl, æt. 21, whose father died of consumption, got a swelling of the size of a walnut, and accompanied by scarcely any pain, in the right inguinal region. The swelling grew slowly to the size of the fist. Her medical attendant, who diagnosed an abscess, kept the swelling poulticed for eight days, and at the end of that time made an incision, out of which a large quantity of pus escaped. In consequence of the rather profuse suppuration which followed, the strength of the patient diminished rapidly; she lost her appetite, and soon got intense pain in the back, which shot down into the right leg, and became so intense in walking and standing that she could not keep herself up any longer. A curvature of the spine having also formed, she came to seek relief in the hospital. On examining her, I found a considerable curvature in the region of the eighth and ninth dorsal vertebræ, with all the characteristic symptoms of suppuration of the bodies of several vertebræ. There was, moreover, the already mentioned suppurating fistula in the groin, into which a probe could be passed upwards for a distance of two inches.

The patient was kept for six weeks on the cold cushion represented in fig. 4. Leeches were applied from time to time in the region of the prominent vertebræ, and cod-liver oil and iron were given internally. Under this treatment at the end of the six weeks all pain and tenderness of the vertebræ on pressure and in walking had disappeared, and although there was still some pus secreted from the fistula, the girl had recovered sufficiently to leave the hospital. This she did at her own wish, but was advised to continue with the internal remedies for some time longer.

This is the place to say a few words about the treatment of the abscesses (the so-called "senkungs-" or "congestions-abscesse") which so often occur in inflammation of the vertebral column, and which point in such various situations. They are not unfrequently the first symptom which indicates to the parents or the medical attendant that the patient is labouring under a vertebral affection. It is my principle

never to open these abscesses, because, in my opinion, it never does any good to the patient, but may sometimes do a great deal of harm. It is well known that such abscesses are scarcely ever painful to the patient, or otherwise inconvenient. For years together they retain the same size, or become alternately a little smaller and a little larger. If they are opened the immediate consequence is a more or less considerable loss of the juices of the body, and, as a rule, the suppuration goes on for a long time, and the secretion of pus, equally with any of the vital processes, exhausts the strength of the body. We have also to consider the danger already alluded to of chronic or even acute pyæmia. In former times I used occasionally to see such patients die rapidly after the abscess had been opened. On the other hand, it is well known that even very extensive abscesses of this kind become not unfrequently absorbed as the patient's general health improves. It is true that there are surgeons who doubt the possibility of any such absorption. This is shown by the long and serious debate on this subject which took place in the year 1858 at the Paris Surgical Society. But such a doubt, I should think, can only exist in the mind of those surgeons who are in the habit of at once plunging their bistouries into any spot in which they detect fluctuation. The duty of the surgeon in these cases seems to me to be, not to accelerate or occasion the opening of the abscess, but to prevent it as long as possible, and in the mean time to do his best to promote absorption. In cases of this kind, also, the continued abstraction of heat seems to be of use, as the following example, to which I could add several others, will show.

Case 12.—*Chronic rheumatic inflammation of several dorsal and lumbar vertebræ; psoas-abscess on the left side; entire absorption of the same under four weeks' treatment by ice.*—John P—, a boy, æt. 16, caught a severe cold three years ago, and in consequence of it complained of pain in the back, which became more intense after long walking or in carrying heavy weights, but which sometimes nearly altogether disappeared. The pain shot from time to time along the intercostal nerves to the anterior surface of the chest, and appears to have been taken for a symptom of pleurisy, and was treated by blistering, &c. A year ago the patient remarked, for the first time, a small, scarcely painful, swelling in the left groin. It grew slowly to the size of a hen's egg, and about half a year ago another swelling appeared somewhat above the first. These swellings were

at the beginning taken for tumours of the glands, and were treated by various domestic remedies. But as they did not diminish, and as the pain in the back increased, the patient came at last to the hospital for relief.

At the time of his admission he presented so healthy an aspect that one would scarcely have suspected that he was labouring under so severe a disease. In his left groin, just below Poupart's ligament, and to the outer side of the femoral artery, there was an oval, tense, and distinctly fluctuating swelling, of the size of an egg. It was not tender to the touch, and was covered by normal skin, which was nether red nor hot. There was another swelling just above Poupart's ligament, and situated somewhat more outwardly than the first, towards the anterior superior spine of the ilium. It was more rounded and somewhat smaller and softer than the other, but very similar in appearance to it. The fluid contents of the lower swelling admitted of being squeezed by slow pressure into the upper one, which, in consequence, became larger and tenser. It was then possible to push the finger into the circular opening existing in the fascia lata, just below Poupart's ligament. It was evident that this was a psoas abscess, in communication with the diseased vertebral column. In the region of the lower dorsal vertebræ there was an angular curvature, which formed an angle of about 155°. The prominent spinous processes were somewhat tender on pressure, and blows on the head or hip produced pain.

The patient kept his bed for four weeks, lying continuously on his back, and during the whole time ice-bags were applied to the vertebral column as well as to the abscess. At the end of this time the latter had entirely disappeared, the vertebral tenderness was quite gone, and the patient left the hospital at his own wish.

It might possibly be objected that in these cases it is not the ice which has promoted absorption, but the improved circumstances under which the patients were placed in the hospital. But I have not unfrequently seen that under the influence of ice abscesses have disappeared which had formed in consequence of acute inflammatory processes; hot abscesses, the opening of which I thought it most desirable to prevent. The following case may serve as an example.

Case 13.—*Hot abscess over the scapula, after contusion; absorption of the matter after three weeks' treatment by ice.*—H. H—, a

girl, æt. 8, of healthy appearance, fell down in the winter while sliding, and knocked her back against the edge of a stone. It was especially the region of the left scapula which was hurt, and she felt much pain there for several hours afterwards. For some days the contused spot continued to be painful, but the child did not complain, as it wished to share in a visit to some relatives living at a distance; it was only noticed to be particularly quiet. On the sixth day the elder sister, in washing the child's back, observed on the scapula a flat swelling, of the size of half a hen's egg, which was tender to the touch, but covered with normal skin. The child stated that it had no pain, but it moved the left arm as little as possible, kept it bent, and supported it always with the right hand. The next day the family set out on their journey to visit their relatives. They drove about twenty-five miles in cold, wet weather, in an open carriage, and the consequence was that the child caught a severe cold. In the evening it complained of pain in the swelling, which had increased in size and had become intensely tender to the touch. The next day a medical man, a friend of mine, was called in, and found the swelling of the size of half a goose's egg, very tender to the touch, covered with red skin, and pulsating. He ordered an ice-bag, which was applied uninterruptedly day and night. Under this treatment the tenderness decreased, but the swelling grew slowly larger, especially upwards. Every evening the girl had an attack of fever, and in the night dragging pains in various parts of the body, especially in the left arm and in the right leg. During the day she was comparatively well. The surgeon being doubtful if the abscess ought to be opened or not, called me in in consultation. I found over the left scapula a distinctly fluctuating swelling, of such a size as just to admit of being covered by my hand. Its upper margin came near to the spine of the scapula; its lower extended about one hand's breadth downwards and backwards, from the inferior angle of the scapula. The lower portion of the fluctuating sac was largest when the patient was sitting down. The skin over the swelling was hot, and the part very tender to the touch. Every movement of the left arm occasioned great pain. When the body was bent forwards, and the fluid in the sac at the same time pressed upwards, the lower angle of the scapula could be distinctly felt on deep pressure, and was found not to be fractured, as I was inclined at first to believe. This was easily ascertained by comparing it with the right scapula. It was, nevertheless, very pain-

ful to the touch. Considering the state of things, I think I was justified in forming the opinion that in consequence of the contusion blood had been at first extravasated, and subacute inflammation developed, which latter had been increased by the child's catching cold; and that the swelling now before me was an abscess, which contained pus mixed with blood, and which threatened to open outwardly. As it was certain that the bone was involved, I thought it important to prevent the opening of the abscess as long as possible; I therefore gave the advice to continue perseveringly with the application of cold, and to give some iodide of potassium internally. Some months afterwards the little patient was brought to me entirely cured. The abscess had not opened, but had been absorbed under the continuous use of ice in three weeks' time. There remained not the slightest tenderness, and the arm could be moved in any direction with perfect ease; even by the closest examination no trace of the previous disease could be detected.

Inflammation of the bursa patellæ is another form of acute inflammation of frequent occurrence, which, according to most surgeons, cannot be dispersed in its advanced stages. Linhart, Dumreicher, and others, recommend the division of the inflamed bursa as soon as possible, for the purpose of giving free vent to the exudation, and thus of shortening the course of the complaint. Stromeyer, also, in his 'Handbook' (p. 581), states that in these cases the inflammation cannot be dispersed even with the help of many leeches, and that poultices are to be applied, and an early opening to be made, especially where the bursa has burst, and has, by infiltrating the surrounding cellular tissue with pus, given rise to diffuse, suppurative inflammation. Formerly I used myself to make an early opening, but now I almost always effect dispersion and resorption of the matter by continuous abstraction of heat, and hasten these effects by the application of some leeches. The case which led me to adopt this mode of treatment was the following.

Case 14.—*Inflammation of the bursa patellæ; application of ice for thirteen days.*—John K—, a working man, æt. 30, accustomed to strong drinks, had worked in the cold weather kneeling on hard ground. From this he got a violent inflammation of the bursa patellæ. When he came into the hospital the front of the knee was enormously swollen and fluctuating. The skin was red over half his leg, and

was hot and very tender to the touch. The leg was at once placed in a raised position on Heister's splint, an ice-bag was put on the middle of the swelling, and round about it twelve leeches were applied. Incipient but distinct symptoms of delirium tremens had set in, and for this the patient was ordered a quarter of a grain of morphia every two hours. After he had in this way taken two and a half grains he fell quietly asleep for twelve hours together. When he awoke he was quite sensible, the inflammatory symptoms had visibly diminished, and continued under the constant application of ice to decrease during the next few days. On the fifth day a red, fluctuating, and painful swelling formed at the outer side of the thigh, about six inches above the patella. This was evidently an abscess, which threatened to break. An ice-bag was placed on this also. The pus, which was certainly present, never escaped externally, but was in both places absorbed. On the seventh day the swelling over the patella had entirely disappeared, on the thirteenth that in the thigh had also gone. The skin over them was pale and wrinkled. On the fifteenth day the patient left the hospital entirely cured.

I also make use of continuous abstraction of heat in many cases of eye-disease. In all cases of injuries of the eye, whether superficial or deep, dry cold is a remedy which is surpassed by no other. Injuries of the cornea, sclerotic, and iris, heal under this more rapidly and with less pain than under any other treatment. In most cases the development of painful inflammation is thus prevented, and reproduction and the formation of cicatrices are much more rapidly effected than if inflammation is allowed to spread. The blood extravasated into the anterior chamber of the eye, in consequence of contusion of the globe, is, under this treatment, absorbed with comparative rapidity, as the following case shows.

Case 15.—*Contusion of the globe of the eye; hyperæmia; application of ice for a fortnight.*—T. H—, a man, æt. 26, came to the hospital with a contusion of his left eye, in consequence of a blow with a whip, which he had received on the previous day. The conjunctiva was much swollen and hyperæmic. The cornea appeared very dull. A third of the anterior chamber was filled with blood. In the texture of the iris there was a speck of extravasated blood, of the size of a hemp-seed. The patient had great intolerance of light,

and could only indistinctly recognise even large objects. He complained of scintillation and pain in the eye and forehead. I made him keep his bed, placed an ice-bag on the eye, had him bled to twelve ounces, and ordered him saline draughts. After the ice had been used continuously, day and night, for a fortnight, the man was nearly well, and at the end of the third week left the hospital completely cured. The extravasated blood had been entirely absorbed, the hyperæmia had disappeared, and the power of sight was completely recovered.

But even in those cases in which the injury has affected the internal tunics of the eye, and in which it has been so extensive that the whole organ must become destroyed by it, by using ice the process of atrophy takes place with very little pain and with scarcely any disturbance of the general health of the patient, which is, I believe, never the case when the affection has been treated by cataplasms. The following case may serve as an instance.

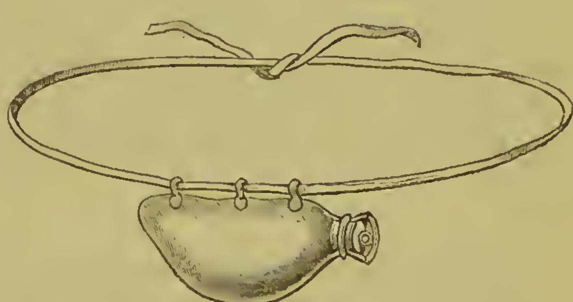
Case 16.—*Destruction of the eye by small shot; application of ice for four weeks.*—F. S—, a strong man, æt. 28, received, on the 28th October, 1855, a charge of small shot in the left side of his face from a distance of three yards. The shot struck the left cheek, in a line extending from the tip of the nose to the mastoid process. The patient lost his sight immediately after he had received the shot. The cheek began to swell rapidly, and the numerous wounds bled freely. When he came to the hospital, two hours after the accident, the whole left side of the face was enormously swollen, especially the eyelids, the cheek, and the left side of the nose, the tip of which seemed to be pushed towards the right side. Many small openings, with blackened margins, were visible on the swollen surface. They were most densely strown just below the left eye, in an area of about an inch in diameter. Some of the wounds were examined with a probe, for the purpose of finding out what direction the shot had taken. We came to the conclusion that some of the shot, which had entered in the neighbourhood of the nose, had gone into the upper jaw, whilst those which had entered the cheek had travelled for a distance through the soft parts, and passed out again in the neighbourhood of the parotis. Some of the shot had struck the ear. None had entered above the eye, but the eye was lacerated, and

the globe had collapsed. In the cornea there was found an opening of the size of a hemp-seed. The conjunctiva and the eyelids were enormously swollen. The patient complained of violent pain in the eye; his pulse was 64. Blood still oozed from many of the openings. A quarter of a grain of morphia was ordered, an ice-bag was placed on the eye, and another one on the cheek. The conjunctiva of the right eye was hyperæmic, and the eye was intolerant of light. This was caused by some grains of powder having entered the conjunctiva on both sides of the cornea. They were at once removed with a couching-needle. On the next day the swelling had spread from the cheek over the neck and breast; the swelling of the lids prevented the left eye from being opened. The lids of the right eye had become œdematous, and an ice-bag was therefore placed on them also. During the next few days the swelling diminished rapidly under the continuous application of ice. On the 31st of October the right eye was found quite free from swelling. The ice was then discontinued. The patient was able now to open his left eye, but he could not see out of it. The anterior chamber was completely filled with blood, so as to prevent the iris from being seen. There was a whitish exudation on the opening in the cornea. The other wounds had begun to suppurate. The patient was very thirsty, had no appetite, but was free from pain. On the 2d of November the left eye became painful. The globe was much swollen, and protruded from between the lids. It was scarified with the scissors, and the swelling and the pain diminished in consequence. On the evening of the 4th of November the lower part of the cheek was considerably swollen. The swelling was hard, and on pressure matter oozed out of all the wounds. Probably the inflammation had spread over the parotis, which had most likely been wounded by some of the shot. The pus which came out of several of the openings was very watery, being, no doubt, mixed with saliva. Some of the fistulæ were now made wider, and a warm cataplasm was applied to the cheek, under which treatment the swelling soon began to diminish. During the whole of this time the ice-bag remained on the eye, and if any attempt was made to remove it the patient at once felt increased heat and pain, but the eye was quite painless as long as the ice was upon it. The patient left his bed on the 14th of November. The swelling of the cheek had much decreased, and there was also much less suppuration. The warm cataplasm was left off. On the 19th of November most of

the wounds were cicatrized; those which were not secreted but little pus. The eye, on which an ice-bag was still kept, was somewhat red, the anterior chamber was clearer, the opening in the cornea nearly closed. A roundish excavation, filled with yellow exudation, was now found in the sclerotic, at the lower margin of the cornea; a shot had probably entered there also. The circumference of the globe was reduced to about half its normal size. On the 29th of November the ice became disagreeable to the patient, and a lead-lotion was used in its stead. The inflammation of the conjunctiva had almost entirely disappeared, and the small wounds were cicatrized. The globe was still very soft, the anterior chamber was clear, but its size was diminished in consequence of the iris having been drawn towards the cornea. Through the pupil the lens was seen, of a snow-white colour. After the irritation of the conjunctiva had been completely subdued the patient left the hospital. Although some shot had certainly lodged in the eye, or gone through it, no suppurative ophthalmia had been developed, and the eye had caused but little pain to the patient. Whoever has witnessed the terrible and long-continued suffering which attends suppuration of an injured eye when the case is treated by cataplasms will, I am sure, appreciate how favorable a course this case took under the treatment by ice.

When, after eye-operations, inflammation sets in and threatens to neutralize their good effects, I make use of cold with the best results. When, after extraction, drilling, or depression of a gray cataract, the part gets more painful than it should normally after operations, I am in the habit of applying an ice-bag to the eye, of the form represented in fig. 7.

Fig. 7.



In most cases the pain disappears rapidly under this treatment, so that bleeding, which remedy I used to employ early, becomes unnecessary. The reason why eye-doctors are generally so much afraid to use cold is that most of them are acquainted with its effects only as shown in the application of cold compresses. Frequently

the eye does not bear moisture well, and the constant changing of the compresses irritates this delicate organ so much that the beneficial effects of cold have no chance of showing themselves. If the eye-doctors would only make use of proper ice-bags, such as, when filled with small pieces of ice, adapt themselves easily and without pressure to the closed eye, they would soon become convinced of the excellent effects of cold in most ophthalmic inflammations. In applying these ice-bags it is best to cover the eye with a piece of lint, and to keep the bag in position by passing a piece of tape through the three india-rubber rings which are attached to the bag, and then tying it round the head. It is true that in inflammation of the deeper structures of the eye, attended by increased pressure within the organ, no favorable results are derived from the use of cold, and that we cannot do without the important remedies which modern ophthalmology has made known to us. But in all forms of inflammation of the more superficially seated parts of the eye, as the lids, the conjunctiva, the cornea, and even the iris, cold is often much more efficacious than repeated bleeding. Catarrh and blennorrhœa of the eye disappear rapidly in most cases under continuous abstraction of heat, and in eczematous inflammations of the conjunctiva and cornea, which are so common, and which are described by authors under such various names (scrofulous ophthalmia, pustular ophthalmia, and so on), no other remedy removes so rapidly as dry cold the inflammatory symptoms and the intolerance of light connected with them. In many cases of the kind, after the patient has been treated and tortured for a long time without effect by tartar-emetic ointment and other painful counter-irritants, I have seen the intolerance of light disappear under the treatment by cold in a comparatively short time. Even those forms of iritis which are produced by the patient's catching cold run a more favorable and rapid course under the treatment by cold than under that by atropine alone, or by other remedies which are efficacious in these cases.[1]

[1] I feel bound to mention that as early as in 1846, when I was a student at Gottingen, I had the opportunity of seeing cold used in most cases of inflammation of the eye which happened to come under the care of Professor Ruete, my esteemed master. He made use of cold with great perseverance, and often with such remarkably favorable results that I was even then convinced that the efficacy of this powerful remedy mainly depends on its being applied in a proper way.

Amongst the prejudices which exist against the use of cold the most common are those which relate to its employment in inflammation of internal and deeper-seated organs, and which are founded on the notion that it is strictly contra-indicated in all diseases which have originated in the patient's catching cold, and especially in the varieties of so-called rheumatic inflammation. The first of these prejudices has been removed in many quarters by the success of the hydropathic doctors, and I am happy to see that Niemeyer, in his excellent 'Handbook of Special Pathology and Therapeutics,' has recently recommended the use of cold in various internal inflammations, especially in pneumonia, croup, and cynanche, in pericarditis and peritonitis. I can corroborate his observations in many points, but am anxious to convince him how much more efficaciously and conveniently cold may be applied in the way I have described than in the form of cold compresses, ice-cravats, &c. As bearing on this subject, I may just mention that my friend and colleague, Professor Bartels, made use of cold, by means of wet sheets frequently changed, in an epidemic of measles which occurred in the course of this year in Kiel and the surrounding districts, as a remedy for the supervening pneumonia, which was very prevalent, especially amongst children of the lower classes. He had better results from this than from any other mode of treatment.

Cold does not act with equally good effect in all forms of cynanche tonsillaris. In those which are marked by dark, erysipelatous redness and slight swelling of the mucous membrane stimulating linaments act more beneficially than continuous abstraction of heat; but, on the other hand, the ice-bag is the best remedy for the cases which incline to the formation of abscesses. I will relate the following case as a striking example. A friend of mine, a man, æt. 40, complained to me that ever since he was ten years old he had suffered in the winter time, at least once, but often more frequently, from violent cynanche tonsillaris, which had led every time, with severe pain, to the formation of abscess, and this had not been prevented by any of the various modes of treatment he had undergone. I advised him, as soon as he felt the next attack coming on, to place an ice-bag on his neck. Soon after this it happened that he was seized by his usual complaint a night or two before an important journey which he had to make. He followed my advice, and under the influence of cold the inflammation went off in a few hours, without formation of abscess. This same rapid cure was

effected several times under similar circumstances, but during the last winter he had no attack of inflammation at all, and is, of course, most thankful for the advice I gave him.

In reference to the other prejudice, that the application of cold is obnoxious in cases of rheumatic inflammation, I most urgently give the advice, henceforth and for ever, to abandon this old prejudice. Probably the fact that wet, as a rule, is not well borne in rheumatic affections, has been the origin of it. In using dry cold, I have never met with the so-called rheumatic metastases which are dreaded by so many medical men. For a long time now I have treated by cold, and with the best results, rheumatic inflammation of single joints or of bones and muscles. I have myself several times during the winter been attacked by painful inflammation of the sheaths of the muscles of the forearm, when, somewhat heated from the performance of bloody operations, I have forgotten to pull my shirt-sleeves down, which I had drawn high up under my light coat, and, thus exposed, have led my pupils through the draughty spaces of the hospital. When, the next morning, I have found the muscles painful and swollen, I have placed an ice-bag for several hours on the tender spot, and the result has been that in a very short time the pain and the inflammation have disappeared.

But also in acute rheumatism of joints, accompanied by fever and metastases of inflammation from one joint to another, I have in several cases made use of dry cold with the best results. Some years ago Professor Stromeyer told me he had observed that acute rheumatism ran a much more favorable course under the cooling treatment than under any other. He has since published his views on the subject in the second edition of his 'Maximen der Kriegs-heilkunst,' p. 25. This, of course, confirmed me still more in the opinion I had formed. Last year a young physician, a near relative of mine, was attacked by violent acute rheumatism. I took him at once into the hospital, and applied cold to every affected joint, and this with a better result than I have ever witnessed from any other mode of treatment. I will give a brief history of the case.

Case 17.—*Acute rheumatism; application of ice for eight days.*—The patient, who was a strong man, æt. 24, had been out boating in the middle of January, 1859, in light clothes, whilst a cold east wind was blowing. The next day the right wrist-joint was somewhat swollen and tender. On the 20th of January both knee-joints

began to swell, and became so painful that the patient had to keep his bed. On the evening of the 21st the left ankle-joint became also affected. He then sent for me. I found him very feverish; and as he had no sufficient attendance at home, I made the proposal to take him into the hospital and to treat him by continuous abstraction of heat. It was a mild winter, and at first no ice could be procured; I was therefore compelled to apply cold compresses for the first twenty-four hours. They were agreeable to the patient, but the constant renewing of them irritated him so much that he could not sleep, although he had taken half a grain of morphia. The next morning the right ankle-joint and both shoulder-joints were tender, and the fever had increased; I ordered him therefore some digitalis. In the afternoon some ice was procured, and ice-bags were at once placed on the most painful joints. The pain immediately diminished considerably, the fever abated, and the patient slept very well the following night. During the next few days most of the large joints were attacked in succession, whilst those previously affected became free. We applied ice-bags everywhere, so far as the small store of ice permitted. The patient felt the greatest relief from the treatment. He had, when quiet, no pain whatever, the pulse became less frequent every day, and on the 30th of January all his joints were free from pain and swelling. In some of them a little stiffness was still felt. I, of course, examined the heart several times daily, with great care. On the morning of the twenty-seventh of January I heard a slight systolic murmur at the apex, but it had completely disappeared by the afternoon.

I have had, since this, several opportunities of treating similar cases in the same way, and have always had reason to be well satisfied with the results. Several of my colleagues have experienced the same; and one of my pupils, Dr. Nissen, in Schönberg, who was assistant in the hospital when the above case occurred, has sent me the history of several cases which he himself has treated successfully in the same way. One of these cases seems to me to be especially fit for publication, because in the course of the disease the patient was attacked by pericarditis, which also was subdued by the use of cold.

Case 18.—*Acute rheumatism; application of ice for twelve days.*—A strong woman, æt. 27, was attacked on the 19th of November,

1859, by rheumatic fever, and at the beginning the only treatment was the application of cotton-wool and camphor liniment to the affected joints. All this time she had extreme pain, and had not a moment's sleep. Dr. Nissen was sent for on the 29th of November. He found the patient very feverish (the pulse being 136), and moaning aloud with pain. The knee, ankle, wrist, elbow, and several finger-joints of the right side were swollen, fluctuating, red, and not only tender to the touch, but violently painful. The left knee was tender, but not yet visibly swollen. The heart was free. Some digitalis was ordered, and a dose of morphia for the night. All the affected joints were surrounded by ice-bags. The next day he found his patient much better. The violence of the pain abated almost immediately after the application of the ice; the woman slept well for several hours, and felt altogether much relieved. The pulse was now only 90, and it was therefore thought unnecessary to order any more digitalis. Those joints which had been treated by ice were now much less painful than the left knee-joint; and the woman implored him to place an ice-bag on the latter also. During the next day or two the right shoulder and the left ankle-joint became affected, and were at once covered by ice-bags. The inflammation of the previously affected joints was rapidly subsiding. On the morning of the 4th of December, the patient had a sudden attack of palpitation and pain in the region of the heart. Dr. Nissen saw her six hours afterwards, and found her very feverish (the pulse being 144). There was a distinct pericardial friction-sound, and the dulness in the region of the heart was considerably increased The heart-sounds were normal. He ordered some digitalis internally, and eight leeches, followed by a large ice-bag to be applied to the heart. On the 5th of December, the patient's uneasiness was much diminished; the pulse was 108, but auscultation and percussion gave the same results as before. The digitalis and the ice were continued. On the 6th of December the pulse was 100; the pericardial friction-sound was less loud; the woman stated that she felt quite well. The symptoms of inflammation had completely subsided in the joints of the right leg, with the exception of a very little fluctuation which was detected in the knee. The application of ice to all those joints was discontinued. On the 9th, all the joints were free, the friction-sound was scarcely to be heard, and the general health was excellent. All the other ice-bags were removed, including that on the heart, instead of which latter a

blister was applied. On the 13th, the patient left her bed; but as some fluctuating swelling appeared in the left knee-joint, she was sent to bed again, and a blister was put on the part. On the 18th, the exudation in the knee had completely disappeared. On listening to the heart, a slight friction-sound was still heard; and another blister was therefore applied. On the 30th of December, no trace of the affection could be detected either in the joints or in the heart.

A case of acute rheumatism which occurred in this hospital, whilst I was away in the Easter holydays of this year, was treated by my assistant, Dr. Meyer, with great perseverance, and ran a remarkably rapid and favorable course.

Case 19.—*Acute rheumatism; application of ice for seven days.*—T. S—, a powerful man-servant, æt. 27, was taken into the hospital on the 15th of April, for inflammation of both knee-joints. The man had had, eight days previously, a fall from a horse, and both knee-joints were in consequence contused; but he had been able to do his work up to the day previous to his admission, when severe pain and swelling of the affected joints compelled him to keep his bed. Both joints were found much distended by fluid. The right knee measured thirty-eight centimeters in circumference, the left thirty-nine. The skin over them was not red, but its temperature was considerably increased. The patient complained of violent pain, which became still more violent on pressure or motion. He was very feverish, the pulse being 140; extremely thirsty, had no appetite, and his bowels had not been opened for three days. The patient was at once put to bed; both legs were placed in a raised position on Heister's splint; four leeches were applied to each knee, and after the bleeding had been kept up for some time, two ice-bags were put on. He was ordered a strong saline draught, in consequence of which his bowels were well opened during the night. On the following day the pain had considerably diminished, the pulse was only 104; but the patient still complained of unquenchable thirst, and was therefore ordered phosphoric acid for drink. The inflammation of the knee-joints was, most probably, not the effect of the contusion merely, but was due in part to the supervention of rheumatism; for during the following night severe rheumatic fever suddenly set in, attended by swelling of several joints and violent palpitation of the

heart. The left wrist and shoulder and the right elbow-joint were first affected. They were found in the morning swollen, red, and very painful. The patient sweated much; his tongue was dry, and he was very thirsty. The pulse was 96. On auscultation and percussion there was nothing abnormal to be detected in the cardiac region. All the affected joints were covered with ice-bags, and as in the course of this and the following day most of the other joints became also affected, there were no less than twenty-one ice-bags used at a time. The patient was ordered 1 gr. of tartar emetic in 6 oz. of water, of which a tablespoonful was to be taken every two hours. He stated that he felt much relief from the ice. On the evening of the following day the pains had considerably diminished. The pulse was 96, there was no more palpitation of the heart, the tongue had become moist, the thirst had disappeared. The swelling of the knees was but slight, and they were scarcely painful even on pressure. On the 19th of April the pulse was 84. Two or three of the joints only remained a little painful; the knees were no longer tender on pressure. The appetite began to improve. The ice-bags were removed, one after the other, from the joints as they got well. On the 22d all symptoms of rheumatism had disappeared. None of the joints was now swollen, and even the knees seemed to have recovered their normal size. Each measured now 36 centimeters in circumference. The pulse was 76, the tongue was clean, the appetite enormous. On the 29th of April the man went out of the hospital perfectly restored, and was able at once to resume his work.

The cases and facts communicated in this paper will serve, I hope, to illustrate and confirm the views which I have ventured to express, at least so far as to direct the attention of my colleagues to a subject which seems to me to be highly deserving of minute consideration and investigation.

I am well aware that the questions which I have here raised are most extensive, and that I have not entered into them profoundly enough, although I may have trespassed too much already on the patience of my readers. The histories of the cases I have given cannot be regarded as furnishing positive proof, but are simply examples to illustrate what I have observed in numerous cases of a similar kind. I have recorded them as briefly as I possibly could, with the wish not to tire the reader.

It seems to me, above all, desirable to make exact observations on the state of temperature and on the changes in the body during the use of continuous abstraction of heat, in health as well as in disease. For some time past observations have been made in my wards on the changes of temperature which take place in the body after severe injuries or extensive operations, but they have not yet led to results sufficiently positive to serve us as a secure basis on which to attempt to establish the points in question. Those of my colleagues who occupy themselves more with the study of those diseases which follow a definite type, and in which the state of temperature has been more closely investigated, might with much more probability of success turn their attention to these questions, if they should feel inclined to give a trial to the constant abstraction of heat in the treatment of those forms of disease.

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